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THE AMERICAN TOAD (*BUFO LENTIGINOSUS* *AMERICANUS*, LeCONTE)

A STUDY IN DYNAMIC BIOLOGY

NEWTON MILLER

CLARK UNIVERSITY

INTRODUCTION

FOR some years it has been my desire to inaugurate a series of university theses aimed distinctively at studying important American species as forces in nature. This kind of work has seemed to me logically the next step in the advance of American natural history. In fact, it is hard to imagine any other line of real advance possible. Species are not discovered, determined, named and classified for the mere sake of making it possible for people to learn their names.

No matter how common the species, when we ask the questions: What does it do in the economy of nature? What position does it occupy in the vital organization of American natural history? What are its relations to human interests? In short, what expression have we of the species as a force in nature? When we ask these questions of the commonest animals, we find ourselves almost as near the verge of human knowledge as with an undiscovered species. No less a man than Darwin himself led off in the field of dynamic biology with his study of "Earthworms and Vegetable Mould." A

large number of living forms await study from a similar point of view.

If we determine accurately the work of an individual and multiply this by the number of individuals, we have an expression of the species as a force in nature. Hence it is true, in general, that the commoner or more numerous a species is, the more important its biological work. We accordingly begin this series of dynamic studies with one of the commonest of American animals.

Of the ten species of Bufonidae now recognized in the United States *B. l. americanus* has the widest range, which includes almost the entire continent of North America east of the Rocky Mountains. It is, therefore, probably the most valuable type of the group.

The insect problem, further, is one of the most important in the entire field of American natural history. It finds partial expression in the annual tax of \$795,100,-000 which insects impose upon our agricultural resources. This is a rigidly conservative estimate made by the department of agriculture and does not include costs of abating annoyance or losses of household goods or those occasioned by spread of human disease. A large proportion of this loss might be most economically prevented by a reasonable knowledge and utilization of insectivorous animals. With all our books, bulletins and talk about it, we have scarcely made a practical beginning in this direction as a people.

In seeking animal allies to aid in the solution of the insect problem we should choose those which will do the work required most effectively and at the same time present the fewest objectionable features. A good many birds are efficient destroyers of insects, but become injurious if their numbers are unduly increased. Many insects are parasitic and predaceous, but, in general, their breeding or regulation is beyond human control. Clearly an animal to be depended upon to hold insects in check should be one which man can breed in any desired numbers and, on account of the enormous reproductive possibilities of insects, a form which he can in-

crease rapidly. We may do much by way of increase of insectivorous birds, and even bats may prove valuable allies. We may be grateful for the help of predaceous and parastic insects, and good work has been done in importing parasites of foreign insect pests, but clearly we need all the assistance we can find; and all the above agencies are scarcely controllable enough to be depended upon.

For an insectivorous animal which conforms to every requirement of the situation, ease of control and rapid increase, non-injurious, in any numbers, an active feeder in abundance and a patient faster in scarcity, the toad stands probably first on the list among American insectivorous animals. Experiments are now being carried on also with the bob white or American quail with every prospect that this form may prove of equal, if not superior, efficiency, and it will carry added values, in food, sport and weed-seed destruction; but this species is rapidly being exterminated from a large portion of its former range, and it will require a long time for methods of propagation and protection to be worked out and become generally known.

A good deal of unnecessary balancing of accounts has been done of late in attempting to calculate the economic value of species from analysis of the foods. In some cases this has yielded results of some value, but, in general, they have been misleading. Even a small percentage of the gross food of a hawk or shrike, for example, if it consists of a valuable species, might render the predaceous species injurious. In the case of beneficial insects, excepting the honey bee which is under human control, if a species will destroy all the injurious insects, it detracts little or nothing from its efficiency, if it take the beneficial insects as well. This principle applies particularly to the toad, which takes everything in the form of an insect, worm or slug that comes in its way.

Instead of filling its stomach four times in twenty-four hours, as Kirkland estimated, Mr. Miller finds that the toad takes but a single meal a day. This is no discredit

to the efficiency of the species. It means, simply, that we should require four times as many to do the work, and, with the number of eggs produced, this offers no difficulty.

Given a pond or even a small pool insured against drying up during the late spring and early summer, and from which natural enemies are eliminated, toads will breed in any desired numbers up to the limits of insect food supply. It would probably, however, be difficult or even impossible to find a single farm in the United States or Canada, although suffering severely from depredations of insects pests—not even the agricultural experiment station farms—which makes any provision whatever for the breeding of toads. In fact, reports so far gathered reveal the fact that farms in the agricultural states of Indiana, Ohio and Illinois are almost entirely destitute of the species. It is probably safe to infer that the toad has been exterminated already from a considerable portion of the cultivated land in the agricultural states. Is this an inevitable result of draining land, and modern methods of tillage? If this be true, can any changes or adjustments be made which will permit the increase of the species? Will the value of the toad's work warrant anything of the kind? Would it pay to establish special breeding places like our present fish hatcheries, or possibly in connection with them?

In order to answer these and many other questions we must have the data of the life and work of the species. To gather these in a manner, if possible, complete enough to serve as a guide and basis for practical action is the purpose of Mr. Miller's work.

THE AMERICAN TOAD (*Bufo lentiginosus Americanus*
LeConte)

The ancestors of the Bufonidæ first appear as fossils in the Oeningen beds below the Miocene Tertiary strata. The group now has a wide range, being found in all parts of the temperate and torrid world with the exception of Australia and the oceanic islands. The greatest number of species is found in the torrid regions. The

Bufonidæ are represented by nine genera and more than a hundred species. *Bufo* takes its origin in the Sonoran subregion, *i. e.*, the southwestern part of the United States. It is the only genus found in the United States and is represented by ten species, most of which belong to the southern states. The species *lentiginosus* and its varieties are found throughout the eastern part of the United States and Canada. Two varieties, *americanus* and *fowleri*, share this region (Massachusetts).

Jordan and Cope class the American toad along with Fowler's as a variety of *Bufo lentiginosus*.¹

My object in this work is to give as completely as possible the entire life and work of one species as normally lived in its own environment. Observations were made continuously throughout the year on the species, *Bufo lentiginosus americanus*, the results of which are given under the heads, Spawning Habits and Seasons; Development, Habits, and Food; Hibernation; and Enemies.

I am indebted to Dr. C. F. Hodge for suggestions while collecting the data for this paper, also for valuable criticisms of the manuscript. He also kindly consented to write the introduction. I wish also to thank Mr. F. E. Chidester for assisting me with the feeding-tests and in obtaining data on the daily life of the toad.

SPAWNING HABITS AND SEASONS

Observations were made for the springs of 1907 and 1908, but if not otherwise stated the data for this chapter refer to the spring of 1907. Mention is made of 1908 when this season varies from that of 1907.

Toads were first seen in 1907 on the twenty-eighth of March and on the thirteenth of April in 1908. An adult male was found in the water on the night of March 29, 1907, but was so numb that he could not give the usual chirp when picked up. From March 29 to April 21 there was a decided drop in the temperature and toads

¹ More recently Miss Dickerson has given these toads specific distinction, *i. e.*, *Bufo americanus* and *Bufo fowleri*.

were not seen again until the night of the twenty-second of April, on which date seven males were found trilling in pond no. 1.² Eight days later when the migration to the ponds reached its maximum, mated pairs were abundant on the streets and elsewhere on their way to the water. Trilling began in 1908 on the twenty-third of April and spawning on the twenty-fifth, ending May 13.

The seven toads mentioned were the first heard trilling this season. They were sluggish and when taken up between the thumb and finger could not chirp, although they made the attempt.

The accompanying curves show the number of toads as well as the number of males and females that were found in pond no. 1 on the night of April 24 and succeeding nights (Fig. 1). Spawn was found here the twenty-

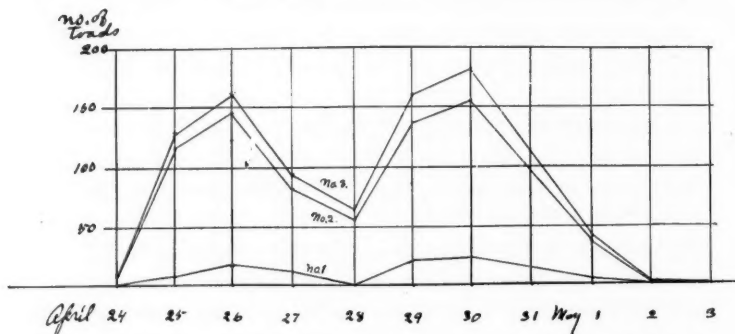


FIG. 1. Curves showing the number of toads in pond, No. 1, during the spawning season. No. 1, Females. No. 2, Males. No. 3, Total.

sixth. The males were most active on this and the night of the twenty-ninth. On these dates they were trilling vigorously and actively swimming about. If they saw an object moving they swam to it with all possible speed. When it was a male, which was usually the case, he was seized, but loosed as soon as he chirped. They even came

² This pond is located at the foot of a small steep hill in a pine grove in Park Avenue Place within a hundred yards of a large permanent pond. It is V-shaped, not more than fifteen feet wide by fifty feet long, and during the spawning season was about eighteen inches deep. For the most part it is filled with leaves, some water plants and debris from a near-by dumping ground. Rains are its only source of supply and it goes dry in summer.

to me as I waded out or made a disturbance in the water. All the females on the night of the twenty-seventh and succeeding nights were removed from the pond to the laboratory.

To sum up my observations on this pond we find: first, that the males are the first to reach the water; second, that there were two waves of spawning activity; third, that not less than ninety-two females visited this pond; fourth, that on an average 88.8 per cent. of the toads in the water at any given time during the spawning season were males.

Of eighty-two taken at random from the streets after the breeding season 60.8 per cent. were females. Kellcott gives the proportion of males to females of those collected along Lake Erie, Ohio, as 175:266; King for toads near Bryn Mawr, Pa., 713:823; *i. e.*, 39.7 per cent. 46.5 per cent., respectively, were males.

The females leave the water as soon as they spawn (from six to eighteen hours after reaching the pond). It is probable that the males return to the ponds night after night, which accounts for their apparent excess in numbers during the spawning season.

Observations were made on five ponds, and it was found that the periods of maximum spawning were not simultaneous in all. In no. 1 and no. 2,³ the spawning period reached its maximum on the nights of April 29 and 30; in no. 3,⁴ May 1 and 2; in no. 4,⁵ May 14 and 15; in no. 5,⁶ May 18 and 19.

³ Pond no. 2 is at the junction of Park Avenue and Maywood Street. It is a permanent pond of about thirty yards in diameter with a depth of three feet. Around its west, north and east edges is a dense growth of bushes, some growing far out into the water. The south side has its growth of flags and water grasses. To the south and west is an open field, while buildings are near to the east and north.

⁴ No. 3 is an overflow of Beaver Brook in an open place just below May Street. This pond is permanent, but the water falls soon after the spring rains to such an extent that most of the spawn is lost.

⁵ The pond in University Park is about thirty yards in diameter and has a depth gradually reaching three feet. Its edges are gravelly and the water plants about it are confined to lilies near the west edge. A row of trees is along the north side.

⁶ The pond located in Elm Park is a permanent, walled body of water

Why should there be this difference of twenty days between the spawning periods of toads in no. 1 and those in no. 2? It can partially be explained by a cold wave which set in on the fourth of May and stopped laying until the ninth. Even so, the toads in no. 1 had finished spawning before the cold wave came. Neglecting the five days of cold weather, there still remains a difference of fifteen days. These ponds are not more than a mile and a half apart. The elevation is practically the same, also the surroundings. Therefore the environments of the two ponds do not seem sufficiently different to account for so much difference in the spawning periods.

My observations indicate that toads proceed to the ponds immediately after emerging from winter quarters. If this be true, then there is a second laying, or some toads do not come from hibernation until late in June. Observations on one toad in my experiments on hibernation suggest that the latter is true. This female came out for the first time May 28, and if she had gone to the water at once, she would have been thirty-three days later than the first that spawned. I might add that I have seen no large toads among these late layers. All the females are about forty grams or less in size.

The bulk of the spawn for the spring of 1908 was laid in all the ponds between May 2 and 6. A minor period of spawning occurred on the eleventh to thirteenth.

The first few warm days of spring bring out the toads and soon the males are heard in the ponds. In a few more days the females follow and spawning begins at once. I have seen no toad eggs in running water, although there is a small stream within less than two hundred feet of ponds where many toads spawned. However, Dr. Hodge informs me that toads spawn in Rock River, Wis., and Ottawa River, Canada, and Mr. Morse states that toads generally spawn in small streams in Ohio. Toads in this region prefer small ponds in which

about two hundred yards long by twenty wide. Its depth is probably three feet. Trees surround it and a three-hundred-foot hill rises at its west side. Buildings skirt all sides of the park except the west.

to spawn. Pond no. 1 is within a hundred yards of a large permanent pond, and when there were a hundred or more toads in the former, very few were to be found about the latter.

In natural ponds toads congregate more or less during the spawning season and in a given pond 90 per cent. or more of the eggs are laid within a radius of fifteen feet. In three of the four natural ponds observed the eggs were deposited in an area less than fifteen feet in diameter. Other places similar to those where the spawn was laid existed in these ponds, but were used only by a very few toads if at all. Usually only a portion of the grassy place chosen was used (Fig. 2). The artificial pond in



FIG. 2. Pond No. 1. Circle shows the spawning place.

University Park gives almost the same results. Here the edges are kept clean and the toads had to deposit their spawn on the bottom. Nevertheless, all the eggs laid here before May 4 were deposited in an eight by ten foot strip of the north shore. Later, May 9-15, eggs were laid all along the north and northeast sides, with a few scattered spawn along the south. The two or three

pairs that may occasionally be found in ponds during the latter part of May and June may deposit their eggs any place about the ponds. I found a little colony of some twenty pairs June 4-8 laying in a space not more than six feet in diameter in an overflow of Beaver Brook. My observations for the spring of 1908 on the same ponds confirm the above statements. The spawn in the various ponds was deposited in almost exactly the same places as the previous year.

Since in general only limited areas are used for spawning it would indicate that there is some choice among toads for spawning grounds. This is done by the males, but without the purpose of having the spawn deposited in such and such a place. They reach the water two or three days before the females and begin their trilling. They seek out each other and repeatedly attempt copulation with one another. In this way all the males in the pond are soon brought together. Even a day or two before the females arrive they are assembled, usually, on some grassy spot out in deep water, to which they return after a fruitless attempt to mate with other males. The females, following the trill of the males, come at last to this circle where they mate and lay. Thus these resting places of the males eventually become the spawning grounds. The observations of Mr. Courtis, later repeated by myself, support the view that the females come to the males.

The spawning place is usually a grassy plot well out in the pond where the dead grass or weeds come to within two or three inches of the surface. In artificial ponds where such places do not exist a shallow place along the edge is used. In the former case the female, as a rule, does not go down to lay, while in the latter practically all spawning is done under water.

Should it turn cold while the toads are spawning they leave the water until the temperature again rises. Meanwhile, they may be found concealed in the grass or leaves, or sunning themselves at the edge of the pond, often the

male still clinging to the female with which he had mated before leaving the water.

As with all amphibians the eggs are fertilized outside of the body. In mating the male clasps the female just back of her fore legs and then places his hind feet upon her thighs. This is the position maintained throughout except at intervals during oviposition. The intervals are about fifteen minutes apart. At such times the female straightens her body, raises her head, and stretches her hind legs backwards. The male hooks his feet between the female's hind legs, thus forming a basket between the feet and legs of the male and the legs and body of the female. (Fig. 3.) In this basket are now

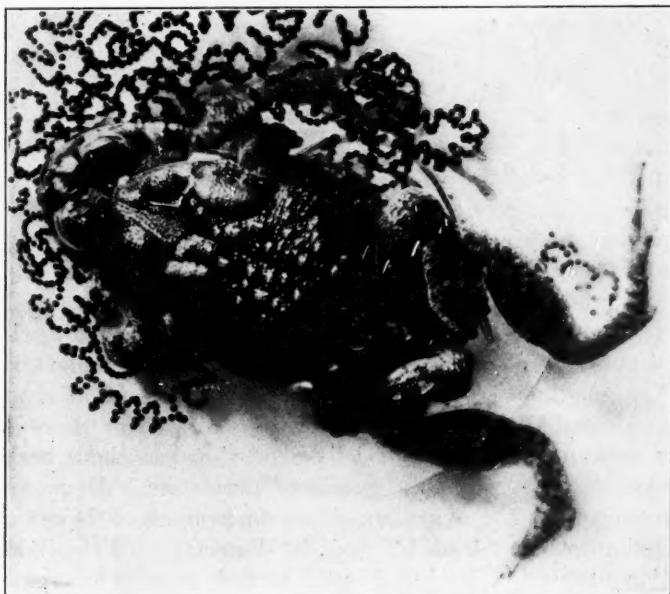


FIG. 3. Shows the formation of the basket in which the eggs are fertilized.

deposited probably two or three hundred eggs, while simultaneously the male ejects his sperm. Boulenger is no doubt mistaken in the purpose of this process when he writes that the male (*B. vulgaris*) aids the female in

oviposition by pulling the eggs strings with his toes. That the male ejects his sperm at this time was demonstrated by microscopical examination of a slide applied to the anus of the male just before and during oviposition. In the former case, very few if any spermatozoa were found, while, in the latter, they were found in abundance.

As soon as the eggs and sperm are ejected the toads remain quiet, holding the mass in the improvised basket perfectly quiet from one to three minutes. This is ample time for the spermatozoa to get well attached to the eggs, since fertilization takes place within four minutes from the time the eggs leave the oviduct. The female now moves along, stretching out the strings of eggs just laid.

Most of the eggs are deposited in water not more than ten inches deep. Those found in weedy and grassy places are, usually, not over four inches under water. In University and Elm Park ponds where the toads were much disturbed, a large number of eggs was deposited some distance from the edge at a depth of eighteen inches or more. This deep laying seemed of no particular disadvantage, since a toad can remain underwater as long as fifteen minutes, which is ample time to fertilize the eggs and stretch out the egg strand.

The eggs are encased in a continuous, cylindrical, gelatinous strand of three to four millimeters in diameter. The gelatinous coat is added in the oviduct, and as soon as ejected absorbs water and swells. In this way there is a mass of .0225 gram of jelly-like substance about each egg, which serves as a means of protection. Eggs removed from the ovary weigh on an average .0024 gram and measure 1.1 to 1.2 mm. in diameter. When laid they are variously spaced in the strands even of the same spawn (Fig. 4). Sometimes they are crowded together so that as many as three may be in the same cross-section, or again regularly spaced at intervals of about 1 mm. The strings may be laid by either twos or fours. I have not observed in smaller toads (under 7 cm.) more than two strands being deposited at the same time, but in the

larger females four are not uncommon. The time required for oviposition varies between six and eighteen hours.

The number of eggs in spawns may be said to vary with the sizes of the toads. Thus females 6 cm. long lay

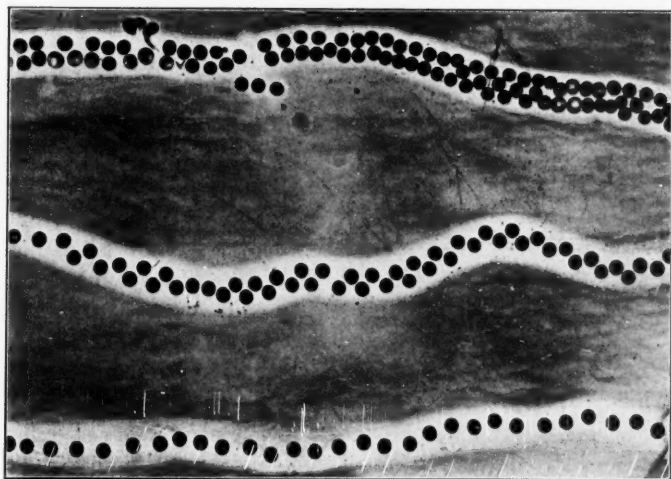


FIG. 4. Spacing of the eggs as seen in a single spawn.

about 4,000 eggs while a toad 9 cm. long may lay 15,000. The smallest spawn observed consisted of 3,929 eggs and the largest 15,835. These were laid in the laboratory.

On making some computations on the largest spawn, we find that the eggs weigh 38 grams and with the gelatinous covering, 394 grams. This latter weight is about five times the weight of the toad. Again, allowing a space of 1.5 mm. for each egg, we get 41 meters, or 134 feet as the length of the strings.

Only the males trill or make any sounds. While trilling the mouth and nostrils are closed and a pouch under the throat is inflated. Trilling in the full strong note was observed only in connection with mating. Miss Dickinson describes the call as follows:

The sustained note is not only high pitched and tremulous, it seems to have a dual character as though a low note was droned at the same

time that the high one is whistled. Imitate the call by whistling the upper and at the same time humming the lower of the following notes: Concerning the call of the toad after the breeding season she writes:

I have heard this feeble note of the toad in August only some half dozen times (the toads were under observation in "moss garden" in the house).

One of the toads in experiment no. II. gave this call April 5, 6 and 22. I heard at least ten toads giving this feeble note on the afternoon of the twenty-first of September, 1907, and at night of the same date just before a thunder storm. In 1908 this trill was heard as late as September 26. This note is not at all similar to that of the breeding season. It is about the same pitch but much weaker, more guttural, and not so long. The trill is not so distinct as in the spring call.

It has already been stated that on an average 88.8 per cent. of all the toads in a pond at any given time are males. Thus, for every female there are seven males. Often a male clasps a female before she reaches the water and she must carry him to the pond. At the pond a lively scene follows until the female with her mate reaches the spawning place. I have often observed two or three males clasping the same female and a few instances of four. In such cases the female is helpless and is usually held underwater. Darwin states on the authority of Dr. Günther that a female is sometimes drowned during the struggle among the males. In these contests the male always gets hold of the female, for he will not hold another male.

The one that first clasps the female in the mating position can not be dislodged, no matter how much stronger his rivals may be. The female, if she is in the water, helps her partner evade the other males by diving or sinking. This she does repeatedly and to good advantage during oviposition.

Males can not distinguish at sight males from females. For this reason they are continually clasping one another. They have a call of three or four notes which they utter in rapid succession when taken up between the finger and

thumb, or clasped by another male. This seems to be a warning signal, for a male will release another as soon as he chirps. In ponds males fight over a dead male as though it were a female. They will clasp anything moving or which touches their breast that they can hold.

It was mentioned above that spawn was deposited at a depth of eighteen inches or more in certain ponds. This depth appeared to be unusual, still the eggs developed normally. The following experiment was made with 500 eggs to test the effect of depth on hatching. One hundred eggs were placed in each of five tumblers over the tops of which were stretched pieces of cheese cloth. These tumblers were then attached six inches apart to a stick which was fastened upright in pond no. 2.

The experiment was started the twenty-first of May. The eggs in the lowest vessel developed much faster than those in the top, apparently as much as a day ahead. This can probably be explained by the fact that a cold wave came the day the experiment was started and continued for four or five days. Thus the surface was cooled more than the deeper water, but the final result of the hatching of the eggs at different depths was the same. A similar experiment made in pond no. 4 gave uniform development for eggs at depths between six and twenty-four inches. From these data it is seen that depth to as much as twenty-four inches does not affect materially the hatching of the eggs.

Counts made on eggs in various places show that 68 per cent. to 90 per cent. are fertile. On an average, development begins in 85 per cent. of the eggs.

DEVELOPMENT, HABITS AND FOOD

The toads leave the water as soon as they spawn. Development proceeds rapidly in the eggs from which tadpoles emerge within two to six days, depending upon the temperature. At this time the larva is about 3 mm. long without mouth or appendages. On the under side of its head behind the buccal area are two small crescent-like depressions known as "sucking disks." These are

glands, however, and it is by their sticky secretion and not by suction that the tadpole attaches itself to objects.

For a day or two after hatching the tadpoles cling most of the time to the gelatinous strings or near-by plants. The tail is noticeably compressed on the second day and



FIG. 5. Toad tadpoles feeding on a pout in a natural pond.

the larva makes excursions from time to time, eventually reaching the shallow water. The mouth is formed in three to seven days, and the larvæ begin feeding. Henceforth the tadpole is a voracious feeder, living largely upon the slime at the bottom of the pond and that collected upon sticks and plants. Meats, fresh or putrid, are eagerly devoured and vegetable matter is also eaten. In this way the tadpoles play the part of scavengers (Fig. 5).

The first gills are external and are soon replaced by internal ones.

The first indication of transformation is the growth of the legs. They appear as small knobs or buds about the twentieth day of the tadpole's life. Both pairs grow rapidly and within ten days may be fully formed. The front legs, concealed in the gill chamber, appear suddenly a short time before the tadpole leaves the water. One is thrust out at the breathing pore and the other breaks directly through the operculum. The tadpole is now almost ready to abandon its aquatic life and within a day it may

do so. A few days previous to this time, the tail has begun to shrink and by the second day out of the water may be completely absorbed. Meanwhile great changes take place in the region of the head. The eyes become enlarged and elevated; the larval beak or jaws are lost; the mouth broadens and a tongue forms; the gill slits close and the lungs mature. The long coiled alimentary canal shortens and differentiates into stomach and intestine. The color changes from a jet black to a brownish tinge with faint spots. The skin is yet perfectly smooth. All of this growth, development and metamorphosis from the egg to the completion of the toad may take place in thirty-two days, or two hundred, as in the case of some kept in the laboratory in a poorly fed condition.

The following table is given to show the rate of growth and development.

TABLE I

	Total Length, mm.	Body Length, mm.	Hind Leg, mm.	Weight, Grams.
April 29, eggs laid.				.0024
May 4 larvae still in the gelatin.	3.5			
" 5 " " " " "	4.2			
" 7 " " " " "	6.0			
" 9 " out of the gelatin	7.0			
" 11 " " " " "	8.0			
" 13 " " " " "	9.2			.0157
" 15 " " " " "	10.5			.0132
" 19 " " " " "	11.0			.0199
" 21 " " " " "	12.0			.0295
" 23 " " " " "	15.0			.0385
" 25 " " " " "	17.0			.0566
" 27 " " " " "	19.5		1.0	.0901
" 29 " " " " "	22.0		2.0	.1310
" 31 " " " " "	23.0		2.5	.1561
June 2 " " " " "	22.4		2.5	.1560
" 4 " " " " "	23.8	9.5	3.0	.1460
" 6 " " " " "	23.4	10.0	3.5	.1570
" 8 " " " " "	24.4	10.0	4.0	.1680
" 10 " " " " "	25.8	10.0	5.7	.1700
" 10 largest specimen.	28.0	10.5	7.0	.2200
" 12 one transformed.	25.2	10.0	8.7	.1520
" 14 " "	10-25.0	10.0	11.0	.1020
" 18 all metamorphosed.	10.0	10.0	11.5	.0796

The weight given for the eggs is that previously determined for those removed from the ovary. To weigh the tadpoles, they were placed upon a glass slide covered with filter paper which was moistened sufficiently to keep

the tadpoles alive. The larvæ, slide, etc., were weighed, and then, removing the tadpoles by means of a dissecting needle, the slide, etc., were again quickly weighed. This method gave me the weight desired without killing the tadpoles. There was of course some evaporation and loss of water in removing the larvæ. But, since it was approximately the same for each weight, the data are not affected to any great extent.

This table is the average weight and measurements of eighteen tadpoles which were kept in a vessel by them-



FIG. 6. Transformation. *a*, day before leaving the water; *b*, just leaving the water; *c*, day after leaving the water.

selves with water plants. Their food was mostly dog-biscuit with a little meat.

Toads just metamorphosed were seen for the first time on the morning of the twenty-third of June. During the next ten days most of the toads of the early spawn abandoned the ponds. The number leaving the water at this time is probably 90 per cent. of all that emerge during the summer. At this time eggs and tadpoles in all stages and toads just metamorphosed could be found in the same pond. Tadpoles were in the ponds as late as August 15. Larvæ from eggs laid April 29th were kept in the laboratory until October 20.

Newly emerged toads are so susceptible to transpiration that they must remain in damp places for a few days. Many remain near the ponds in the grass, under stones, etc., during the day, thereby escaping the fate of those wandering away too soon. At nights or on cloudy days during the time that tadpoles are transforming, the vicinity of ponds may be alive with young toads migrating from the water.

The great numbers that leave the water or places of

hiding during a rain, have led to the belief that toads are rained down.

After abandoning their aquatic life, these little toads simply eat, grow and endeavor to escape their enemies. By the first of October, their weight at the time of metamorphosis has been increased more than sixty times. In other words, they double in weight almost every sixteen days.

Reports indicate that toads are very unequally distributed both as to the country in general and to local areas. Killicut found them abundant along Lake Erie in Ohio. Here in Massachusetts they are very numerous. Through the northern half of Indiana toads can almost be said to be scarce, while in the southern half they are found in considerable numbers. Few were observed in the vicinity of Waterloo, Iowa. Reports from Wisconsin and Minnesota indicate that this species is well represented in these states, while in Maine and Ohio they are said to be abundant.

Toads are more numerous in and about towns than elsewhere. Very rarely is a toad seen in a large field which is under cultivation. Only fifty toads were seen during a whole season on one thousand acres of farming land in central Indiana. This scarcity may be accounted for by two factors, *i. e.*, first, that pasturing and tillage kill the toads or, second, that the extensive drainage has exterminated the toad by depriving it of breeding places. Sufficient data are not at hand, as yet, upon these two points of general and local distribution to draw conclusions.

During the summer the toad leads a solitary life and no more utters the ringing trill of the breeding season. On very rare occasions, which may be as late as September 21, he may strike up a weak, shorter and more guttural note, which is much inferior to the vigor and fullness of the mating call. This note is so rare that the toad may be considered silent throughout the summer.

Rarely, except at night or on cloudy days, is the toad seen hopping about. Late in the fall, however, he may

be found sunning himself in the grass. At this time he is sluggish and apparently only waiting for cold weather to put him to sleep. Toads come out of their hidings on rainy days, and, especially, on nights following rainy days. I counted fifty-two toads in a vacant lot between 7:00-8:00 P.M., July 22, after a hard rain in the afternoon. Counts repeated here for the next nine days at the same time in the evening varied between 0 and 16, and most of the times there were less than six toads to be seen. On the twenty-second many of this year's toads were seen, but on the following nine days, scarcely one could be found. In general, throughout the summer the toad passes the day in concealment in damp sheltered places, while the night, the toad's day, is spent in foraging.

Some of the toads in the above observation of July 22 were shedding their skins, and many appeared to have just done so. This process is frequent and often takes place immediately after the toad comes from its place of concealment.

The one in experiment IV shed her skin every seventh to eleventh day, or on an average once every 8.2 days from October 23 to April 8. Those in Experiment III did not shed so often nor as regularly; some going as long as four weeks without shedding, unless they did so in their burrows. I have seen some indications that shedding is done while the toad remains buried, and the process may be frequent and regular although the toad remains concealed.

The process is accomplished by pushing the skin forward to the neck by the hind feet and then pulling it off over the head by the fore feet. That on the hind legs and feet is rubbed off against the sides of the body, while that on the fore legs and feet is pulled off by the jaws. The skin is removed from the feet and legs in a manner very similar to that of seizing a glove by the top and turning it wrong side out as it is pulled off. As the skin is raked over the head it is seized by the jaws and swallowed. Previous to the removal of the skin, the new skin secretes

a viscid substance that loosens the old from the new and leaves the body moist for a short time after the old skin has been shed. The whole process is accomplished in about two minutes.

The following is a record of the work done by an adult male (weighing 36 g.) in twenty-seven hours. The distance traveled in this time was 85 feet, and the number of insects taken, 23. Twenty hours and five minutes were spent in burrows and, for the rest of the time, the toad was active only one hour and forty-seven minutes. It is probable that the period of activity from 7:20-8:40 A.M. would have been lessened had not another toad entered the burrow of the one under observation and forced him out. More than half of the total distance traveled in the twenty-seven hours was covered in quest of a second burrow. The following is the time spent in the burrows.

July 25, 1:50 P.M.-7:55 P.M.

July 25, 11:50 P.M.-2:30 A.M.

July 26, 4:05 A.M.-7:20 A.M.

July 26, 8:40 A.M.-4:00 P.M.

The food taken consists entirely of animal matter. Mineral or vegetable material is often found in the toad's stomach, but it is there by accident. I have found as much as 0.07 gram of coal and sand in the stomach of a 5.5-gram toad; it had just swallowed its skin and along with it the coal and sand which stuck to it. I have also seen pieces of grass, winged seeds and other bits of plants snapped up and swallowed with insects.

The amount a toad will eat in twenty-four hours seems somewhat astonishing if we were to judge from various reports. Miss Ellen M. Foscett records one feeding of a large toad as ninety to one hundred rose beetles. She also adds that "the toad showed no signs of wishing to conclude his meal." Kirkland found "the remains of seventy-seven myriapods in a single stomach, fifty-five army worms in another, sixty-five gypsy moth caterpillars in a fourth. In these cases, however, but one kind of food was present and the toads were above the usual size." He states further, on the authority of Mr. J. E.

Wilcox, that a toad ate twenty-four fourth moult gypsy moth larvæ in ten minutes; again, on authority of F. H. Mosher, that a toad ate thirty to thirty-five full-grown celery worms in three hours at intervals of about twenty minutes. Eighty-six house flies are recorded by Dr. C. F. Hodge as being snapped up in less than ten minutes. Dr. J. R. Slonaker fed an eighty-gram toad all the beefsteak and flies it would eat for two days. On the first day, December 7, it ate 2.8 grams of beefsteak and three flies; on the second, 3 grams of beefsteak and twelve flies. From further feeding he concluded that his toad would eat on an average forty flies per day.

A. H. Kirkland, H. Garman and F. A. Hartman have given us much valuable information on the food of the toad from careful analyses of stomach contents. Kirkland examined one hundred and forty-nine stomachs collected from April to September, of which he gives the following as a fair specimen. The toad was killed at 9:00 P.M. May 11, 1896, and found to contain: "9 ants (*C. Pennsylvanicus*), 6 cutworms, 5 myriapods (*Julius* sp.), 6 sowbugs (*Parcello* sp.), 1 weevil (*Hylobius pales*) and 1 carabid beetle (*Pterastichus* sp.)"; total 28. Kirkland's report shows that 84 per cent. of the toad's food consists of 28 per cent. lepidopterous insects and their larvæ, 27 per cent. beetles, 19 per cent. ants, 10 per cent. myriapods. The insects alone were represented by eighty species.

The five records which follow are those of Garman.

No. 20, a toad $1\frac{1}{8}$ inch long, obtained on the Experiment Farm, August, 1896. 2 *Drasterius elegans*, 1 *Diabrotica 12-punctata*, 1 *Sytene tæmata*, 1 millipede (*Polydesmus*), 1 bug, fragments of ants, 6 chinch bugs and fragments of others.

No. 1, a toad of medium size, captured beneath an electric burner, Lexington, Ky., Oct. 1, 1894, had eaten 27 ants, 19 sowbugs, 3 spiders, 1 caterpillar, and 10 plant-lice; total 60.

No. 2, a toad 1 inch long, captured in a celery patch, Lexington, Ky., Sept. 2, 1891, had eaten 14 ants and 1 caterpillar; total 15.

No. 3, captured in a strawberry patch, Lexington, Aug. 5, 1890, had eaten 2 large ground beetles, 1 tiger beetle, 1 corn root-worm beetle, 1 lady beetle, 8 small ground beetles and 9 ants; total 22.

No. 4, taken Lexington, July 26, 1890, had eaten 2 Colorado potato

beetles, 1 click beetle, 4 bugs (Cydnidae), 1 tiger beetle, 1 moth, 7 ground beetles, 6 ants, 1 millipede and 1 sowbug; total 24.

The above data on six stomachs are given in order to show what a toad feeds upon while in its natural habitat. So far as known a toad will eat any arthropod, mollusc or worm that it can easily swallow, without regard to spines, stings, noxious odor or taste. The question then is, in determining the economic importance of the toad's feeding habits, not to know how much it can or will eat, but to know, *how much it actually does eat*. Owing to the slow digestive rate of the toad, stomach examinations give us a close approximation to the desired information.

It is stated by Kirkland that a toad will fill its stomach four times per day. This seemed to me to necessitate rather rapid digestion for a cold-blooded animal, and I made the following experiment to test it.

August 8, a female weighing 35 grams was killed thirteen hours after having fed on three Colorado potato beetles and two grasshoppers. Examination of the stomach showed the beetles still entire. The grasshoppers had disintegrated, but none of the food had passed from the stomach.

August 26, 2:00 P.M., two toads, nos. 1 and 2, weighing seven and twenty-five grams, respectively, were fed chiefly on grasshoppers. Eighteen hours later, no. 1 was killed and at the end of twenty-two hours, no. 2. All of the food was still in the stomach of no. 1 but well digested. Stomach no. 2 contained a portion of the food, but most had passed into the intestine.

August 30, 11:30 P.M., a toad weighing twenty grams was fed on two beetles, two grasshoppers and one fly. When examined eight hours later the stomach showed: the grasshoppers well digested, but not disintegrated and the fly and both beetles still entire.

These tests demonstrate that a toad can not fill its stomach more than once in twenty-four hours. A toad will eat at intervals when its stomach is only partially filled, but to completely fill it four times a day is out of the question.

TEST No. I

Date, 1907.	Toad No. 1.		Toad No. 2.		Toad No. 3.		Toad No. 4.		Toad No. 5.		Toad No. 6.		Toad No. 7.		Toad No. 8.		Toad No. 9.		Toad No. 10.	
	No. of Insects.	No. of Insects, grams.	Insects.	grams.	Insects.	grams.	Insects.	grams.	Insects.	grams.	Insects.	grams.	Insects.	grams.	Insects.	grams.	Insects.	grams.	Insects.	grams.
Aug. 3	40	3.5	37	3.8	60	2.8	15	1.2	29	3.0	42	3.9	30	3.0	28	3.0	27	3.2	40	3.8
" 4	60	3.7	37	3.8	2	1.2	0	0.0	0	0.0	0	.0	15	1.4	40	2.5	0	0.0	0	0.0
" 5	0	0	15	4.1	13	2.5	0	0.0	2	.11	14	1.6	18	1.3	0	0	0	0.0	0	0.0
" 6	25	2.1	22	2.5	37	3.1	0	0.0	0	0.0	0	0.0	69	1.6	19	1.3	0	0.0	17	.67
" 7	30	3.0	42	3.5	27	1.4	0	0.0	11	.22	0	0.0	37	2.4	0	.0	0	0.0	0	0.0
" 8	6	1.9	36	2.6	45	3.8	0	0.0	0	0.0	0	0.0	14	1.4	0	.0	0	0.0	63	0.7
Totals.	161	141	189	16.0	184	14.0	15	1.2	42	3.0	56	3.7	183	10.7	113	9.1	27	3.2	130	5.1

TEST No. II

Date, 1908	Toad No. 1, Weight 71.		Toad No. 2, Weight 64.		Toad No. 3, Weight 49.		Toad No. 4, Weight 46.		Toad No. 5, Weight 50.		Toad No. 6, Weight 37.		Toad No. 7, Weight 33.		Toad No. 8, Weight 35.		Toad No. 9, Weight 26.		Toad No. 10, Weight 16.	
	No. of Insects.	No. of grams.	Insects.	grams.	Insects.	grams.	Insects.	grams.	Insects.	grams.	Insects.	grams.	Insects.	grams.	Insects.	grams.	Insects.	grams.	Insects.	grams.
June 19	30	1.5	45	2.0	36	1.5	15	0.5	39	0.6	165	3.8	42	1.7	0.0	0.0	40	1.6	17	0.7
" 20	25	1.7	152	5.1	28	1.8	38	2.8	35	1.4	80	3.2	39	1.3	0.0	0.0	21	0.6	38	1.7
" 21	34	1.3	23	1.5	33	2.3	50	2.0	60	S	40	S	21	S	0.7	3.5	13	0.6	49	2.7
" 22	S	1.8	3	1.1	43	3.2	2.7	2.7	25	4.1	0.5	0.5	23	1.8	2.6	1.7	1.1	1.1	35	1.7
" 23		5.7	2.5	2.5	4.0	4.0	3.0	3.0	3.6	3.6	0.5	0.5	S	1.4	1.5	1.5	2.6	2.6		1.7
June 24	Insects could not be obtained on account of rain.																			
" 25	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X	X
" 26		0.3	0.5	0.5	0.0	0.0	0.8	0.8	0.5	0.5	0.0	0.0	0.0	0.0	0.3	0.3	0.6	0.6	0.2	0.2
" 27	S	0.0	2.0	2.0	0.0	0.0	0.2	0.2	0.0	0.0	0.1	0.1	0.0	0.5	0.1	0.1	0.0	0.0	1.0	1.0
Totals.		12.5		15.0		12.8		12.2		16.3		10.6		7.4		6.8		7.1		10.2

TEST NO. III

Date. 1908.	Toad No. 1. Weight 49.		Toad No. 2. Weight 50.		Toad No. 3. Weight 45.		Toad No. 4. Weight 40.		Toad No. 5. Weight 33.		Toad No. 6. Weight 34.		Toad No. 7. Weight 28.		Toad No. 8. Weight 24.		Toad No. 9. Weight 27.		Toad No. 10. Weight 42.	
	No. of insects.	No. of grams.	insects.	grams.	insects.	grams.	insects.	grams.	insects.	grams.	insects.	grams.	insects.	grams.	insects.	grams.	insects.	grams.	insects.	grams.
July 23		3.0		3.5		0.5		0.3		1.3		1.5		2.0		2.5		1.6		2.1
" 24		2.0		2.7		0.2		0.6		1.1		1.5	S	1.6		D		D		0.0
" 25		D		D		0.5		0.9		D		0.0		2.4		D		D		D
" 26		0.0	S	D		0.0	S	2.5		0.0	S	2.4		4.5		3.1		D		D
" 27		0.2		3.6		D		2.0		2.1		1.2	S	3.7		2.5		2.5		2.7
" 28	S	0.8		D		D		1.8		D		0.0	S	D		D		D		2.0
" 29		D		4.6		D		2.0		1.6		1.7		2.4		D		D		1.6
" 30		0.6		D		D		D		1.0		0.4		D		1.2		1.5		1.5
" 31		D	S	2.0		D		0.2		0.5		0.0		1.8		D		D		2.0
Aug. 1		D		D		D		0.8		D		1.6		D		0.5		D		D
" 2		D		D		D		D		D		D		1.0		D		D		D
" 3		0.8		D		D		D	S	D		1.8		0.4		D		D		D
" 4						D														
" 5						D														
Totals.		7.4		16.4		1.2		11.2		7.6		12.1		19.8		9.8		7.5		11.9

Average amount eaten per day, 11.2 grams.

Average weight of the toads, 36.6 grams.

Average amount eaten per day per toad, 1.12 grams.

S = times toads were observed shedding their skins.

D = days toads remained buried.

0.0 = days toads refused to eat when not buried.

The results given in the following table are of feeding tests made in order to see how much and how often a toad will eat. The tests were made with sweepings and, as grasshoppers formed the greater part, the experiment was carried on largely with them. This, I think, does not affect the experiment to any great extent, since toads show very little preference, if any, for certain species of insects. If toads are placed in cages with sweepings, various species are taken at random, provided only that they be moving and of a size easily swallowed.

Ten toads, half males and half females, were chosen for each test. Each toad was confined separately in a glass cage ($4 \times 5 \times 7$ inches) in which was about two inches of moist earth. Throughout test no. I, insects were kept constantly before the toads. The insects were weighed and counted, then put into the cages with the toads. In test no. II the toads were allowed to feed, one at a time, in a wire cage containing insects. The cage with the insects was weighed before the toad was put in and then weighed again after the toad was removed. Allowance had to be made for loss by evaporation in determining the amount eaten. A more satisfactory cage was constructed entirely of glass for test no. III. It measured $8 \times 10 \times 10$ inches and had on its bottom a half inch of dry sand for the absorption of moisture. Evaporation from this cage was less than a gram in ten hours. Insects were placed in it and the process of weighing followed as in test no. II.

In nos. I and III the toads were not disturbed unless they were out of the earth. But in no. II they were dug up every day, if necessary, in order that they be offered food at least once.

The following are the results of the three feeding tests.

The data show that for test no. II the toads feed oftener than in the other two experiments, but that the amount eaten, in proportion to the weight of the toads, is 3 and 2 per cent. less than in tests nos. I and III, respectively.

The time represented in these experiments is equal to that of one toad for 260 days, 96 of which were passed

without feeding. In other words, a toad refuses food on 36.9 per cent. of the days. Therefore instead of a toad filling its stomach four times in twenty-four hours, according to Kirkland, it may eat (probably fill its stomach) once in a day and a half. The average amount eaten per toad per day in these 260 days is 1.12 g. The average weight of the toads is 36.6 grams, which is about the weight of the largest males or of a medium-sized female.

These feeding tests show that the amount eaten at one feeding compares very closely with the stomach contents as recorded by Garmann, Hartman, and Kirkland. Furthermore they demonstrate: (1) That the stomach of a toad is not a rubber bag with an unlimited capacity as commonly supposed; (2) that the toad when feeding, if food is abundant, soon fills its stomach; (3) that toads do not feed every day. This was suspected, since toads are seen in greater numbers on rainy nights.

Usually as soon as the toads fed they buried themselves and remained so from one to ten days. Sometimes their eyes and nostrils were left exposed. Even so, insects placed in their cages usually failed to tempt them to leave their burrows.

If we take the mean, 36.6 grams, as an average sized toad, we find that it eats on an average, 26 insects or 1.12 grams per day. Counting May, June, July and August as a toad's feeding months, it will eat in this time some 3,200 insects, or 134.4 grams.

To estimate the value of a toad's work in dollars and cents is rather difficult, since the toad eats beneficial as well as harmful insects. Garman from his data does not hesitate to class the toad with useful animals, yet he would not have us overlook the number of beneficial insects eaten. According to Kirkland eleven per cent. of its food consists of insects directly or indirectly valuable to man and eighty per cent. is either directly injurious or obnoxious. He computes on the data previously given of a stomach content that a toad is worth \$19.88 per year for the cutworms alone which it destroys. He assumes that a toad fills its stomach four times a day with 6 cut-

worms and other insects for three months and that the cutworms do damage to the amount of one cent each. Granting that cutworms are injurious to the amount of one cent each and that they work four months instead of three, still Kirkland's estimate is about four times too large, for a toad can not fill its stomach more than once in twenty-four hours. Furthermore, my experiments show that it eats, on an average, only once in one and a half days. These factors give the toad a value of about \$5 per year on the basis of Kirkland's estimate. Such figures may be approximately correct for greenhouses, gardens or truck farms, but on the whole I am inclined to think they are too large for farming districts.

(To be concluded)

NOTES ON THE BEHAVIOR OF THE DOMESTIC FOWL¹

PHILIP B. HADLEY

RHODE ISLAND AGRICULTURAL EXPERIMENT STATION

DURING the month of February, 1909, the writer's attention was called to an interesting feature of the behavior of a buff rock cockerel kept in the poultry department of the Rhode Island State College at Kingston, Rhode Island. Having briefly described the facts of the case to Dr. Robert M. Yerkes of the Department of Psychology of Harvard University, and having been informed by him that the behavior in question was somewhat noteworthy, the writer has collected data, secured photographs, and desires to present the following description, believing it may be of interest to students of animal behavior. The point of behavior under consideration consists in the acquired habit of an adult barred rock cockerel of "working" an automatic feeder, containing bran and grain, located in one of the colony houses of the poultry department, and by this action of securing a larger amount of the grain constituent than the feeder, working by itself, would naturally be able to supply.

The automatic feeder in question is of a type manufactured by John Anderson, of Slocums, Rhode Island, and involves mainly a box (Fig. 1, *B*) containing feed, and a tray (*T*) suspended beneath it. The dimensions of the box are roughly sixteen by eleven by eight inches. The food material escapes from the box through a narrow opening at the back and bottom of the feeder, and is caught in the tray, which is thirteen inches long by seven and one half inches broad, and is suspended eleven inches from the floor. The supply of food material falling on the tray is regulated by a balance system. The tray is so

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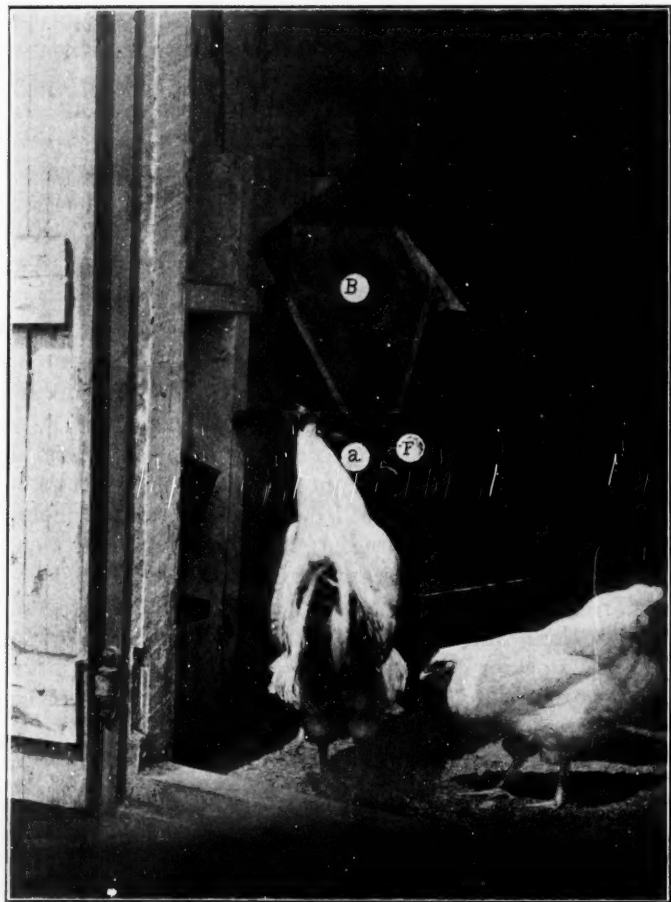


FIG. 1.

suspended from the point *F* that, when it is lightened (consequent to feeding), it rises and simultaneously admits a new supply of food material from the box. As soon as a sufficient amount to increase the weight of the tray has fallen, the latter drops again and automatically shuts off the supply. This process of filling and empty-

ing continues as long as there is material left in the box and there are fowls to eat from the tray.

The material contained in the automatic feeder is usually a mixture of bran, wheat, and whole corn. While both the hens and the cockerel eat a fair amount of the bran constituent, they like the grain, especially the whole corn, much better. This predilection is exhibited especially by the cockerel, which attempts to obtain a minimum amount of the bran and a maximum amount of the corn. Usually there are from four to eight hens, together with the cockerel, attempting to feed from the tray at the same time. As a consequence, the grain, which is mixed with the bran, is in the tray but a short time before it is eaten, and nothing but the bran remains. When this condition has come about the cockerel proceeds to "work" the feeder. This is accomplished in the following manner.

As shown in Fig. 1, the cockerel while eating almost invariably stands at the left-hand corner of the feeder. At this point he usually feeds from the corner of the tray, while the hens feed from both sides or from the ends, some of them standing between the feed-tray and the wall, where there is a free space of ten and one half inches. As soon as the corn has all been eaten the cockerel steps back sufficiently to clear the corner of the tray, takes a few steps forward around the corner, raises its head high and gives a vigorous push or peck to the beam designated (*a*) in Fig. 1. The force of this blow usually serves to throw out the board slat which holds the food back in the box; and, as a result, fresh material falls upon the tray, the increased weight of which immediately closes this outlet from the box. As soon as more corn has fallen on the tray the cockerel hastily returns to his former position at the corner of the tray and devours as many of the kernels as possible before the hens have again cleared the receptacle of grain; whereupon the cockerel again leaves the tray and pecks the beam. The final result of this continuous performance is that the tray be-

comes so heavily stocked with the bran constituent that it will not longer work automatically, sometimes becoming so heavy that even the most vigorous pecks of the cockerel no longer serve to raise it and admit more food.

The movements of the cockerel in working the feeder are, as a whole, characterized by directness and accuracy. Occasionally, however, there is a slight hesitation in striking the beam. In such a case the bird leaves the tray and turns toward the beam, but returns to the tray before having struck, as if influenced by the thought that grain had already entered the tray or that the other birds would secure it first. This hesitation is usually followed, however, by the complete successful performance. The accuracy of the strike is noteworthy, and a deep groove has been worn on the edge of the beam at the point where the beak strikes.

The cockerel mentioned above is a fine specimen of the buff rock variety. It was hatched at the Rhode Island College Poultry Plant in February, 1908, and now measures twenty-three inches in height. The bird has been confined in the colony house (with appended yard) for about four months, and the type of behavior described had been observed about two months before it came to the notice of the writer. It thus seems probable that the bird had been in the house with the feeder accessible for about two months before the trick was learned. At the time of writing (April 5, 1909) the cockerel still has access to the automatic feeder and works it regularly every day.

Since making the observations recorded in the preceding paragraphs the writer has secured other data which show that the ability to work the automatic feeders is not restricted to the previously mentioned buff rock cockerel alone, but is also possessed by several of the hens located in the same colony house, and by another white Orpington cockerel in an adjoining poultry house. While the behavior of the white Orpington cockerel is in most respects similar to that of the buff rock, the action of the buff rock hens is somewhat different. When the

hens feed from the tray, a few of them, as has been remarked, stand back of the automatic feeder, *i. e.*, between it and the wall, a space of about ten and one-half inches. In this position the feed tray is slightly more than breast high (thirteen and one-half inches) at the top of the rim, while slightly above the head of the birds (nineteen inches from the floor and six inches from tray) runs a guide-board, eleven by two and one-half inches. This board, which really serves as a guide to the food falling from the box into the feed-tray, is so hung that if it be struck from behind outward the effect is the same as that of striking the beam (*a*) at the point designated; that is, the grain is let into the feed-tray. Striking the guide-board from before does not accomplish this result. Now it appears that several of the hens have learned the trick of operating the supply by striking from behind, with their bills, the guide-board designated above. To accomplish this the bird merely needs to raise its head from the tray and to stretch its neck slightly in order to reach the board which is sharply pecked. It appears, however, that the hens do not attempt this manœuvre so often as the cockerel performs the other. This is perhaps for two reasons: first, the hens do not care so much for the grain; and secondly, the cockerel is apparently always quite ready to save the hens the trouble of working the feeder for themselves.

How long was required for the hens to learn the trick of operating the feeder is not known; nor are any data at hand regarding the proficiency shown at successive stages in the process of learning. When the performance was first observed by the writer the technique was already perfected. It would be interesting to learn how, in the experience of the cockerel, the connection was first made between the movement of the beam and the presence of fresh corn on the tray. The matter would be easier of explanation were the action in question one which, on the first performance, could easily have been accomplished by chance, as, for instance, the case of the dog or cat acci-

dentally striking the latch when leaping or pawing at the door for admittance. In the present instance there is performed an action which requires the cockerel to leave temporarily the primary object of his attention, and to perform a manœuvre which it seems could hardly have been hit upon accidentally, while the bird was engaged in the process of feeding.

Since writing the above paragraphs the writer has made other observations which add new interest to the subject in hand. In order to prevent the buff rock cockerel from working the feeder a piece of sheet tin was nailed to the beam which had been struck. This was placed over the beam in such a way as to protect the latter on the top, where the cockerel had usually struck it, as well as on the sides. This sheet of tin extended downward to within about one-eighth of an inch of the bottom of the beam, and hung about one-fourth of an inch outside of the same. Thus the whole of the beam, except one-eighth of an inch of the lower margin, was protected from the beak of the cockerel, and it was expected that this precaution would effectually prevent all further "working" of the feeder. This result, however, was not forthcoming, for within two days after the sheet of tin had been placed on the feeder the cockerel was found to have modified his behavior to suit the new unfavorable conditions. Now, instead of attempting to strike the beam from above, as previously, he reached with his beak underneath the projecting sheet of tin, and, grasping the lower margin of the beam, bore it downward with a kind of pulling motion from beneath. This movement, it is apparent, is somewhat different from the one first described. It is, moreover, the only means by which the feeder could possibly be worked by manipulating any accessible part of beam. It is barely possible to consider the second manœuvre as a direct modification of the first, although a different part of the beam is struck, and with a different movement on the part of the bird. It is diffi-

cult to see how this second reaction could have been developed as a result of an accidental success, especially when it is borne in mind that the manœuvre was first initiated and fully perfected within two days of the time when manœuvre number one was discontinued. Whether manœuvre number two is a modification of number one, whether it arose from imitation (which is doubtful, since no person or other fowl is known to have performed the action before it was perfected by the cockerel), or whether it is the result of a chance success following random movements, the writer is not prepared to state. Notwithstanding the fact that manœuvre number two is so different from number one, it is perhaps more reasonable to assume tentatively the correctness of the first hypothesis. But this does not explain the origin of manœuvre number one, although it surely demonstrates a very effective modifiability in the behavior of the cockerel. In the present instance it is also noteworthy, perhaps, that when the first manœuvre was prevented by the presence of the sheet of tin, the cockerel did not profit by an imitation of the hens, which continued to work the feeder from behind as in the first case mentioned above.

Believing that there are certain theoretical considerations arising from the foregoing observations which would be of interest to comparative psychologists, the writer has requested Doctor Yerkes to add hereto comments that seem pertinent to the question in hand. The writer would also take this occasion to express his thanks to Mr. Roy H. Waite, Assistant in Biology, for preparing the photograph which accompanies this paper; also to Mr. Holden, of the College poultry plant, for some of the data here presented.

COMMENT UPON DR. HADLEY'S OBSERVATIONS

Neither casual observation nor systematic experimentation with the domestic fowl has furnished many instances of the type of behavior described by Dr. Hadley. Consequently his observations have considerable interest and value for students of animal behavior.

It is scarcely necessary to say that the most serious defect in this account

of the behavior of a cockerel is the lack of the history of the formation of the habit. This defect is the author's misfortune, as well as the reader's, but it is not his fault, inasmuch as the habit was practically perfect when he first observed it. To attempt to interpret the behavior in psychic terms or to point out its significance would be futile in the absence of definite knowledge of the conditions which originally gave rise to it. This is but one of the innumerable instances in which we need more facts instead of speculation.

Dr. Hadley has suggested that the connection between pecking the beam of the feeder and obtaining grain could not have been discovered accidentally. Should he succeed in proving the truth of this supposition he would make an important contribution to animal psychology, for it is fair to say that the habit arose either from "accident" or from ideas. Evidently the author himself suspects that ideas, or some other forms of psychic process, had to do with the acquisition of the food-getting act. I wish the facts were adequate to prove or disprove this possibility. They are not, in my opinion, and it is therefore out of place to discuss them from that point of view.

The reader can but wonder whether the hens which operate the feeder acquired their method of obtaining grain under the influence of imitation. The conditions apparently furnish an excellent opportunity for the experimental study of imitation in fowls.

Surely the wisest and safest, if not the most satisfying, comment which the student of animal behavior can make concerning Dr. Hadley's observations is "let additional facts appear before interpretation is permitted."

ROBERT M. YERKES.

VITALITY OF PINE SEEDS AND THE DELAYED OPENING OF CONES

PROFESSOR W. C. COKER

UNIVERSITY OF NORTH CAROLINA

ON a visit to California in July, 1908, my curiosity was aroused by the remarkable retention of the still unopened cones in *Pinus attenuata* (*P. tuberculata*) the knob-cone pine, and to a somewhat less conspicuous degree in the Monterey pine (*Pinus radiata*). Trees of *Pinus attenuata* may frequently be seen several feet in diameter and thirty or forty years old, still retaining unopened all the cones they have produced during their lives, the lowest cones circling the tree within hand's reach from the ground. As all cones are borne on new growth it is obvious that as the branches increase in thickness the peduncles of the cones must be broken loose from their connection with the wood, so as to allow the cones to be pushed out by the annual growth, or the cones will be covered as the tree develops and finally imbedded in the wood. As the cones of *P. attenuata* are narrow at the base and thus more easily caught by the annual layers, the latter alternative sometimes occurs and the cones are covered by the growth of the tree.

The cones that remain on the surface of the trunk and branches have no organic connection with the tree, and their peduncles, which are almost an inch long, may be twisted out like a cork from a bottle. It is a well-known fact that in this case the cones never shed their seeds until the tree or branch that bears them dies.

This remarkable peculiarity is exhibited to almost as great a degree by *Pinus radiata* (Monterey pine). Of this tree J. G. Lemmon says:¹

¹ *Sierra Club Bulletin*, Vol. 2, p. 74, 1897.

Trees four and five inches in diameter may be seen on Point Pinos, still retaining every cone they have produced, circling the trunk and limbs from base to apex. Of course the lumber is perforated with holes, the channels formed by the cone-stems on their many years' journey from heart to bark.

Other species of western American pines whose cones are serotinous to a greater or less degree are *P. muricata*, *P. contorta*, *P. contorta* var. *Murrayana* (the lodge pole pine) and *P. chihuahuana*. Of *P. muricata* Lemmon says:² "The cones have been known to remain unopened for twenty or thirty years, then to release good seeds," but he says in another place of the cones of the same tree:³ "They usually open at the time the leaves at the same point fall away from the stems." The *Gardener's Chronicle* for April 24, 1909, gives a good illustration of this pine showing old unopened cones, and in the same number, Mr. W. J. Bean says: "Some of the trees at Kew bear cones which must have developed more than a quarter of a century ago."

Of the eastern American pines the only ones to retain their cones unopened after maturity are the jack pine (*P. Banksiana*) of the north, the Table Mountain pine (*P. pungens*) of the Alleghanies, the pond pine (*P. serotina*) of the southern states, and *P. clausa* of the gulf coast and eastern Florida. In the case of the last species the cones may become imbedded in the wood as in *P. attenuata*.⁴

That this remarkable habit of cone retention is of use in the struggle for existence, at least under the peculiar conditions that exist in our western country, is believed by a number of observers. The explanation that is usually offered is well expressed by John Muir in "Our National Parks" page 104. Speaking of *Pinus attenuata* (under the name of *P. tuberculata*) he says:

² "Handbook of West American Cone-bearers," 3d ed.

³ *Erythea*, Vol. 2, p. 160, 1894.

⁴ In *Garden and Forest*, Vol. 10, p. 232, Professor C. S. Sargent remarks that cones of *P. muricata* often become imbedded in the bark, but in a letter to me he says that this "appears to be erroneous."

This admirable little tree grows on brushy, sun-beaten slopes, which from their position and the inflammable character of the vegetation are most frequently fire-swept. These grounds it is able to hold against all comers, however big and strong, by saving its seeds until death, when all it has produced are scattered over the bare cleared ground, and a new generation quickly springs out of the ashes.

This statement of Mr. Muir's implies that all or a large part of the seeds produced during the life of the tree are capable of germination when shed, and this seems to be the opinion of others (see Lemmon, as quoted above, under *P. muricata*).⁵

Now it is a well-known fact that pine seeds as a rule are very perishable (seeds of *P. palustris* will not germinate, according to my experience, the second spring after their maturity) and it is important to test by actual experiment to what extent seeds retain their vitality under such conditions. In looking over the literature I can find but one experiment that has been made to enlighten us on this point.

In 1874 Dr. Engelman collected a branch of *Pinus contorta* from Colorado (the plant being probably var. *Murrayana*, or lodge pole pine) and after keeping it four and a half years, he sent it to Professor C. S. Sargent, of the Harvard Arboretum, to test the seeds. Professor Sargent planted the seeds in 1879, and his results, as reported in *Bot. Gazette*, Vol. 5, p. 54, 1880, were as follows:

⁵ The reference to *Pinus radiata* by Vernon Bailey on page 34 of C. Hart Merriam's "Results of a Biological Survey of Mount Shasta, California" (*North American Fauna*, 16, 1899) would indicate that its seeds have a hard time on Mount Shasta. He says:

"The trees were loaded with cones, in whorls of three to seven around the branches, and down the trunks to 10 or 12 feet from the ground. Some of the cones must have been 20 or 30 years old, and perhaps much older. I cut off a lot of the old lower cones to see if the seeds were good, and put them on a boulder and cracked them with a few hard blows of the ax. All of them were full of worm dust, with only now and then an undiscovered seed or a fat white worm. Cones of medium age (5 or 6 years back from the end of the branch) were invariably occupied by worms and worm dust, and usually contained few good seeds. Cones only 1 or 2 years old were rarely wormy. A great many of the old cones had been dug into by woodpeckers, either for seeds or, more likely, for the fat white grubs that live on the seeds."

Seeds of 1865 and 1868 did not germinate.

1869 24 seeds planted, 4 germinated.

1870 25 seeds planted, 4 germinated.

1871 6 seeds planted, 2 germinated.

1872 19 seeds planted, 5 germinated.

1873 9 seeds planted, none germinated (cones probably not mature).

This experiment shows that at least some of the seeds of *P. contorta* (var. *Murrayana*?) are capable of germination after retention in the cones for nine or ten years.

My interest having been aroused in this subject while in California, I was led to observe more closely the cones of our native *P. serotina* on my return to South Carolina and it was soon found that the cones of this species often remain attached and unopen for a much longer time than ever reported. In his "North American Silva," Vol. 3, p. 117, Michaux says:

The cones arrive at maturity the second year, but do not release their seeds before the third or fourth.

Sargent follows this statement in his "Silva" and Britton says (in "North American Trees") that the cones "remain closed for several years before dropping the seed." In the neighborhood of Hartville, South Carolina, it was not at all uncommon to find cones that had remained unopened for ten or even more years, and the opportunity was taken to collect cones of different ages for a test of the viability of the seeds. The cones were taken to the New York Botanical Garden and there the test was made in June of this year. Seeds that were obviously blasted or dead (as shown by floating in water) were discarded, and are recorded as "rejected"; only apparently sound seeds were planted. The seeds were first germinated between filter paper in *Sphagnum* moss for about five days until the radicals appeared. A count was then made and the result recorded in the columns of June 29 in the table below.⁶ All the seeds, whether germinated or not, were then planted in soil in pots, and

⁶ Cone No. 1 was not included in this count because its seeds were by mistake planted in soil before the count was taken.

the seedlings that appeared were counted on July 12^r and July 22, with results as shown in the table below.

Years Old.	Rejected.	Planted.	June 29.	Germinated.	
				July 12.	July 22.
3	31	32	?	27	28
4	10	14	6	9	9
4	6	15	13	9	9
6	7	57	30	40	39
6	0	62	52	51	52
6	7	60	58	53	48
7	3	88	42	50	44
8	7	49	10	34	33
8	5	27	2	15	18
8	3	42	0	31	33
9	5	34	3	2	0
9	2	31	10	16	7
14	32	61	33	24	21
14	2	67	7	11	11

Increased numbers in the later readings are due to delayed germinations: decreased numbers to failure to emerge or to damping off after emergence.

It should be noted that the conditions that exist in these serotinous cones are almost ideal for the preservation of the vitality of the seeds. While some exchange of gases is allowed, the spores of fungi and bacteria are effectually excluded; and most important of all, a sufficient humidity is maintained to prevent a fatal dehiscence. That this humidity is due to contact with the moist wood of the live tree is shown by the mechanical opening of the cone through drying when it is removed from the wood, or when the tree dies. This opening, however, is not always either prompt or complete.

^r This counting was, in my absence, kindly made by Mr. Fred. J. Seaver.

THE AFFINITIES OF THE ECHINOIDEA

AUSTIN HOBART CLARK

THE recent discovery that the crinoids in their ontogeny increase the number of their ambulacral post-radial ossicles by the interpolation of numerous ossicles (in pairs) between the first two primitive brachials and the radial, forming what are known as "interpolated division series," as well as by the addition of brachials in a linear series at the growing tip of the arm, where only heretofore addition to the number of the brachials was supposed to occur,¹ has shown that in the manner of increase of the number of ambulacral segments there is a close similarity between the crinoids and the echinoids, both groups adding new plates between those already formed and the radials (oculars), whereas in the ophiuroids and the asteroids new plates are added only at the tip of the arms, not, however, at the extreme tip, as in the crinoid arm, but just proximal to a permanent plate; and the question naturally arises, can the commonly accepted view regarding the interrelations of the various classes of the Echinodermata be maintained in the light of the present state of our knowledge?

Só long ago as 1821 J. S. Miller remarked on the similarity of an inverted *Cidaris* to a crinoid, and this similarity was also noticed by Lovén. That this similarity is not superficial but in reality fundamental has become increasingly evident to me during the course of my studies on the echinoderms, and I have now no hesitation in stating that the crinoids and the echinoids have much more in common, and are much more closely related to each other, than either group is to the asteroids or the ophiuroids.

Considering only the external skeleton, we find that, in the crinoids and echinoids (1) the ambulacra in-

¹ *Proc. U. S. Nat. Mus.*, Vol. 35, No. 1636, pp. 113-131.

crease by the addition of plates proximally between the youngest plate and the radial (ocular); no permanent terminal plate is present; (2) the ambulacral plates always alternate in position; (3) the so-called infrabasals are inconstant (crinoids) or entirely wanting (echinoids and many crinoids); (4) imbrication of ambulacrals is more or less constant; (5) the ambulacrals are on the surface, so that the nerves, water vascular system, schizocœl, etc., are on the inner side, protected by them; (6) there are no definite or constant accessory plates in the ambulacral system; (7) the oral skeleton, when present, can not be directly derived from the ambulacral system; (8) the interambulacral plates are in more or less regular columns; but they always start from a single plate; (9) the interambulacral plates extend laterally outward from the ambulacrals, forming a closed capsule; (10) the plates of the ambulacral system encroach regularly, when at all, upon the peristome, which remains round; (11) the spines are long and usually slender, and are attached to round and prominent spine bosses (present among crinoids in one genus only); (12) the ambulacrals are directly continuous with, and in the same plane as, the radials (oculars): on the other hand, in the ophiuroids and asteroids (1) the ambulacrals increase by the addition of plates distally between the youngest plates and the permanent terminal; (2) the ambulacrals are always opposite each other (a possible exception in certain palæoasteroids); (3) the infrabasals are ontogenetically constant; (4) there is no imbrication of the ambulacrals; (5) the ambulacral ossicles lie deep, so that the radial structures are outside of them; (6) definite and constant adambulacral ossicles or lateral shields are present; (7) there is a peculiar oral skeleton of modified ambulacral plates; (8) the interambulacral plates are in regular rows, starting as some multiple of two; (9) the interambulacral plates enclose more or less the ambulacral plates; (10) the plates of the ambulacral system encroach irregularly upon the peristome, making it more or less sharply stellate; (11) the ambulacral plates

never bear spines; (12) the ambulacra are not continuous with, nor in the same plane as, the so-called radials or terminals.

This would be sufficient in itself to convince any one that the crinoids and echinoids formed one well circumscribed group, while the asteroids and the ophiuroids formed another similar group, entirely distinct; but the "soft parts" furnish abundant additional evidence leading to the same conclusion.

In the urchins and the crinoids the anus is always well developed and functional, while in the asteroids and ophiuroids it is absent, or, if present, does not serve as an exit for refuse matter; the crinoids and urchins have a large and definite peristome which is circular in shape and more or less filled with dermal plates; in the asteroids and ophiuroids the peristome is very much reduced, stellate and without dermal plates; in the crinoids the ambulacral plates are united by ligaments externally and by two parallel rows of muscle bundles internally; in the echinothurids, which alone among the echinoids have a flexible test, the plates are united by more or less ligamentous connective tissue, and within the test there are five pairs of muscle sheets, inserted along the two outer edges of the ambulacral series; the asteroids and ophiuroids have two pairs of muscles, a dorsal and a ventral instead of one pair and a dorsal (external) ligament. In the crinoids and echinoids the intestinal canal is narrow, tubular, without marked sac-like expansions, and always lies in coils of which there may be as many as four; in the asteroids and ophiuroids the digestive system runs direct from the mouth to the anus (when present) without convolutions, but has sac-like widenings, and may have branched radial outgrowths. In the crinoids and the echinoids the gonads are connected with the axial organ in the young, but not in the adult; in the asteroids and ophiuroids they are connected with the axial organ throughout life. In the echinoids and crinoids the pseudo-hæmal system is closed on all sides; in the asteroids and ophiuroids it connects by means of numerous

small apertures with the body-cavity, and, at one point in the pseudo-hæmal ring, with the axial sinus. A blood vascular system is doubtfully present in the ophiuroids and asteroids, but well developed in the echinoids and crinoids. In the echinoids the axial sinus surrounding the stone canal, originally a part of the cœlom, is in open communication with the ampulla into which the madreporite opens. This is comparable to the condition in the crinoids where the madreporic pores open into the body cavity more or less opposite the openings of the stone canals, but is quite different from the condition found in the asteroids and ophiuroids.

Having now shown that the crinoids and the echinoids are closely related, it remains to be seen how an homology may be drawn between the skeletal elements of the two. This is not nearly so difficult a performance as might appear at first sight. The primibrachs of a crinoid represent the first four ambulacral plates of an urchin, which have slipped in between each other so as to lie in a single row; in this single row of four plates the second has disappeared, as shown by the synarthry,² while the third and fourth have united to form an axillary. All the plates in the crinoidal post-radial series up to the third brachial of the free arm represent the ambulacral series of the urchin; the long and tapering crinoid arms from the third brachial onward are homologous to the auricles and apophyses of the urchin, which have become turned outward instead of inward, have become interiorly united, and have increased enormously in length. The crinoid stem is the central (sur-anal) plate of the urchin; originally free, the crinoids first became sessile through simple attachment by the central dorsal plate; this gradually increased in thickness, becoming a thick stalk, like that of *Holopus*; later, owing to the increasing length, fractures were developed transversely, and finally the long jointed crinoid stem resulted.

It has been urged, from their radiate structure, that the echinoderms were primarily fixed; but I can not see

² *AMERICAN NATURALIST*, Vol. 43 (October, 1909), pp. 577-587.

why the octopus could not be assumed to have been descended from stalked ancestors along the same lines. It seems to me that the echinoderms are rather like the bivalve molluscs or the crustacea which contain both free and fixed types, the former in the great majority. The crinoids are the only recent fixed echinoderms; but in the fossil crinoids, as Lang pointed out in *Marsupites* and I independently showed in *Uintacrinus*, there are forms which exhibit no evidence of ever having been attached; in fact the evidence is quite the other way. In these forms the centrale may be, instead of the centro-dorsal, really the dorso-central, in which case we should get an interesting homology with the echinoids.

The association of the holothurians with the echinoids, and hence with the crinoid-echinoid stem, seems to me to be abundantly justified. The following classification of the echinoderm groups is proposed as showing the interrelations of these groups better than any of the synopses previously published.

Phylum ECHINODERMATA

I. Subphylum ECHINODERMATA HETERORADIATA.

A. Pelmatozoa.

1. Crinoidea.
2. Cystidea.
3. Blastoidea.

B. Ovozoo.

1. Echinoidea.

C. Vermiformes.

1. Holothuroidea (*Bohadschoidea*).

II. Subphylum ECHINODERMATA ASTORADIATA.

A. Ophiobrachiata.

1. Ophiuroidea.

B. Stellarides.

1. Asteroidea.

In this table the sequence of groups must not be taken to represent a phylogenetic line; in no class of animals is a phylogenetic sequence more difficult of conception than in the echinoderms. While the heteroradiate echinoderms are, judged by ordinary standards, more perfect than the astoradiate, judged from the echinoderm point of view solely, they can not be considered so well developed as the latter.

THE EARLY BREEDING HABITS OF AMBLYSTOMA PUNCTATUM

ALBERT H. WRIGHT AND ARTHUR A. ALLEN

CORNELL UNIVERSITY

CASUAL observations made at Ithaca, during the past eight or nine years, upon the habits of *Amblystoma punctatum* have emphasized the need of an intensive study of the early breeding habits of this species, and consequently during the spring of 1908 considerable attention was given to this phase of its life history.

The best collecting ground for this species was found to be along the eastern border of the marsh at the head of Cayuga Lake where it is skirted by a state road and by the tracks of a trolley line. Here it is necessary for the salamanders from the hill, on their way to suitable breeding grounds, to cross the tracks and in so doing many are killed by the passing cars. Heretofore, it was believed that the majority came from the ravines which cut through the hill in this locality. To test this, a trap was placed at the mouth of one of the culverts under the road. The trap yielded only eight specimens during the ten days in which the species was migrating. It would, therefore, seem that the salamanders came mainly from the hill itself. For the study of spermatophores and eggs ponds on the hilltop near the university proved most fruitful. Six ponds were visited daily. In these, countless spermatophores and several hundred bunches of eggs were deposited. A chart of each pond was prepared and upon this, for future reference, the position of several areas of spermatophores and each bunch of eggs were indicated with the date of deposition.

The first appearance from hibernation for this species from 1903 to 1908 follows:

1903, March 13.

1904, April 1.

1905, April 1.

1906, March 28.

1907, March 24.

1908, March 23.

In 1908 the first appearance occurred the evening of March 23, when the maximum and minimum air temperature, were 47° and 42° F. The water temperatures of the two ponds under observation on the following morning were 42° and 48° F., respectively. On the above dates of first appearance from 1903 to 1908, the U. S. Weather Bureau Station at Ithaca obtained the following maximum and minimum air temperatures:

Year.	Max. Temp.	Min. Temp.	Year.	Max. Temp.	Min. Temp.
1903	52° F.	31° F.	1906	51° F.	32° F.
1904	55	36	1907	61	43
1905	64	37	1908	47	42

From this it is obvious that a temperature approximating 50° F. or more almost invariably caused the species to emerge.

To verify this conclusion, a careful record of the migration across the railway in 1908 was kept and a summary follows in tabular form:

Date.	Temperature		Number Captured Alive at Night.	Number Killed by the Cars.	Total.
	Max.	Min.			
March 24.	49°	22°		1	1
March 25.	37	19			
March 26.	65	37		15	15
March 27.	63	36	9	13	22
March 28.	72	41	31	24	55
March 29.	41	30	} "cold, cloudy weather with snow flurries." ¹		
March 30.	48	33			
March 31.	43	27			
April 1.	48	27	17		17
April 4.	50	18	3 (in the trap)		3
April 6.	62	40		1	1
Total,			60	54	114

In this table as in the preceding 50° F. appears the normal effective temperature of migration. The crest

¹ Climatological Report for March, 1908. New York Section of the Climatological Service of the Weather Bureau in Cooperation with Cornell University. By W. M. Wilson, p. 19.

of the migration in 1908 came when the maximum air temperature ascended to 60° or 70° F. In two other years the same conditions obtained as shown in the accompanying data:

Date of Crest.	Max. Temp.	Min. Temp.
1901, April 21.	72°	42°
1901, April 22.	86	62
1907, March 27.	70	45
1908, March 28.	72	42

The males begin the migration, as the following table will indicate:

Date (1908).	Number Killed.	Number Captured Alive.
March 26,	15 males.	
March 27,	5 males, 4 females.	6 males, 3 females.
March 28,	10 males, 14 females.	12 males, 19 females.
April 1,		7 males, 10 females.

Thus we see the males began the migration. The next evening, the females appeared in small numbers and the third evening they predominated and continued thus for the remainder of the migration.

In 1908, the migration each evening across the railway began between 7:30 and 8:00 o'clock. By means of an acetylene lamp, the salamanders were observed crawling along close to the rail, often following it for some distance. At times they were seen attempting to cross the track by raising themselves erect on their tails. Even with this aid, scarcely more than the head came above the rail. In this position they often remained until crushed by the passing cars. Generally, however, they followed along the rail until they came to the joints, where the projecting bolts enabled them to work their way over.

The evening upon which spermatophores were first deposited, two salamanders were seen "nosing" each other, and one of the two depositing spermatophores. Neither was captured, and so to determine if this were a part of the regular courtship of the species, several salamanders collected along the track were taken to the laboratory

and placed in jars as follows: Jar No. 1, seven males; No. 2, several each of males and females; No. 3, a male and a female. These specimens had not reached the water, and were carried to the laboratory in a dry bag. The temperature of the water in the jars was 65° F., or fifteen to twenty degrees warmer than that in the ponds.

The seven males of jar no. 1 showed no excitement and deposited no spermatophores until a day or two later. Then, the total of spermatophores for the whole seven was much less than that deposited by the single male of jar no. 3.

In jar no. 2 the males and females became excited the moment they were placed together and many spermatophores were deposited at once. This experiment was repeated the following evening with a different set of salamanders and the males began to deposit spermatophores within fifteen minutes after being placed with the females. No eggs were deposited earlier than two days after the deposition of the first spermatophores.

In jar no. 3 the male, at first, showed no signs of excitement, but upon coming in contact with the female, he became very restless, and "nosed" her about in a definite manner. It seemed to be the object of the male to bring the top of his head in contact with the venter of the female. The throat region of the female seemed to be preferred, although he often began in the cloacal region or even at the tip of the tail and rubbed the dorso-lateral part of the head along her whole ventral side. After each performance of this kind, the male swam away and grasped one of the sticks with its hind legs, bringing the cloaca close to the stem. The tail quivered for a moment and, with an arching of the region just caudad of the cloaca, the vent was lifted from the spermatophore. Then, he immediately returned to the female and began again the "nosing" process. The time consumed in depositing a spermatophore varied from 3 to 16 seconds, the periods for thirteen consecutive depositions being: 5, 3, 6, 10, 13, 12, 7, 11, 12, 16, 11, 10, 10 seconds, re-

spectively. In this way twenty-two spermatophores were deposited in 45 minutes.

Most of this time the female remained quiet. Three times, however, she slowly moved over a spermatophore until the vent rested upon it. Then the hind limbs closed about it. In this position the female remained, each time, for ten to fifteen seconds and apparently made no effort to take any portion of the spermatophore into the cloaca as does *Diemyctylus*. It seemed to us rather that there was a simple passage of the spermatozoa from the spermatophore into the cloaca of the female.

Evidently, then, the female must be present at the time the spermatophores are deposited and in this we find the explanation for the delay in deposition of spermatophores after the species has first appeared from hibernation. The males begin the migration but no spermatophores are deposited until the arrival of the females. In the spring of 1908 the migration began the evening of March 26, but spermatophores were not recorded until the morning of March 28, after the arrival of the females. With the arrival of the females and the ensuing courtship, spermatophores are deposited. These are usually found in stagnant or slowly moving water, four to twelve inches deep, though they are occasionally recorded in water one and one half to two feet in depth. They occur in groups numbering from 2 to 125, covering an area of one half to three feet square. The usual number in a group is between 30 and 50. The spermatophores and the spermatozoa recently described by Smith² need no discussion here.

After the first spermatophores are deposited, an interval elapses before the first eggs are recorded. In former years this interval has varied from a few hours to seven days and we had been led to believe that the females did not come to the ponds until their eggs were ripe. This belief is no longer tenable. After the cloacal

² Smith, B. G., "The Breeding Habits of *Amblystoma punctatum* Linn.," AMERICAN NATURALIST, XLI, No. 486, June, 1907, pp. 381-385.

inclusion of the spermatozoa several hours or days may elapse before the eggs ripen and ovulation occurs. "In 1903 one interval of 2 days was recorded; in 1904, one of 4 days; in 1906, one of 6 days; in 1907, 4 days in one pond, 5 in another, and 7 in a third."

"Egg-laying generally begins about the first of April. In two or three of the last eight years, eggs have been noted before that date. In this period, the earliest record is March 20, 1903. In 1901 they did not begin depositing eggs until after the middle of April. The egg-laying for the species may extend over a month or more. Rarely do we find fresh eggs after May 1. In 1907 our latest record for fresh eggs is April 30; our latest for fresh spermatophores in the same pond, April 27."³

The number of eggs in a complement varies from 130 to 225. These may be deposited in one to ten bunches, two or three per female being a fair average. There is not necessarily uniformity in the size of the bunches, for one female was known to deposit two bunches, one of 140 eggs, the other of 32.

At least thirty minutes are usually consumed in depositing a normal bunch of eggs. One female in depositing a bunch of 140 eggs remained beneath the surface for over an hour. During this time she neither strove to get into a position where she could keep her nostrils out of the water, nor did she once arise to the surface for air. As in other cases, the eggs came out slowly without apparent effort or straining, sometimes but four in a minute. During deposition she was motionless, except for occasional slow movements of the tail. Immediately after deposition, however, for two or three minutes, she swayed back and forth vigorously to disengage herself, for the fresh jelly stuck to the under side of her tail and cloaca. Ten hours later another bunch of 32 eggs finished the complement.

³ Wright, A. H., "Notes on the Breeding Habits of *Amblystoma punctatum*," *Biological Bulletin*, Vol. XIV, No. 4, April, 1908, p. 286.

NOTES AND LITERATURE

MARINE BIOLOGY

Papers from the Tortugas Laboratory of the Carnegie Institution of Washington, Vol. I, 191 pp., Vol. II, 325 pp. (Carnegie Institution of Washington Publications No. 102 and 103, 1908.)

The first collection of memoirs published in the name of the laboratory itself, from this practically tropical marine station, is highly creditable to the students who made the researches, to the director of the laboratory, Dr. A. G. Mayer, and to the great Institution which, on the financial side, has made the studies possible. Still more it is an impressive embodiment of the perception and conception that the sea is a vast, inexhaustible mine of the raw material out of which biological science is constructed; and that this material can be transformed into finished, useful product only on the ground to which it is native. Such writings drive home the truth with special force that would we really know nature we must go where nature is; we must study her *in her home*.

The two volumes contain nineteen papers written by fourteen naturalists, and the range of topics is almost as wide as the field of marine zoology. Yet nearly every one of these papers contains something, some of them many things, that a biologist who daily breathes the air of a large, expansive biological philosophy will want to make memorandum of for future use.

The apparel of the matter presented approaches, though does not reach, perfection. The most serious defect in the outer garments is the lack of aid to general consultation. Not only is there no alphabetical index, but the list of papers at the beginning of each volume is without page references, so one must hunt through the volume for any paper he may want to consult. The principle of the greatest good to the greatest number is against allowing scientific books, especially, to go out wanting such useful incidentals as these. The numberings and letterings of some of the illustrations are too small and indistinct to be easily read by artificial light.

Defects in some of the inner garments are more unfortunate than any in the outer. It is not necessary as one of the authors seems to think, to "investigate into" a subject. To just investi-

gate it is enough. "Further observations are needed . . . especially with reference as to whether," etc. Why the "as to"? This sentence is bungling. But worse than bungling is the statement that "organic data function" in controlling, etc. Such expressions as this can not be let off on the plea of hasty composition, imperfect proof reading, established usage within a special field, or something of the sort. They mean hazy thinking. Literary form is not a vital thing especially to technical science. Nevertheless, considerable attention to it pays in the long run for effort in this way is promotive of clean, clear thought.

It seems to the reviewer that the word "reaction" is being overworked by some students of animal behavior. What more is there in a medusa's "fishing reaction" than in its plain fishing? And what is the gain in speaking of a bird's alighting on a stake as a "reaction"? There is a quality seemingly possessed in some degree by all minds, that tends to accept a new name for an old familiar phenomenon as in some way more explanatory of that phenomenon than the old name. Anything that encourages this tendency is not good for objective science. More, perhaps, in science than in any other domain of knowledge is there need of vigilance against bondage to words.

The subjects treated in the collection may be ranged under these heads: *Cytology*, *normal development*, *regeneration*, *faunal zoology* and *animal behavior*. Some of the papers fall exclusively under one head while others contain matter belonging to two or more. Such notice and comment as can be made here will be ranged under these headings.

Cytology: H. E. Jordan ("The Germinal Spot in Echinoderm Eggs") concludes that the chromosomes in *Echinaster crassispina* are derived from the nucleolus, and that they arise inconstantly in different species of echinoderms from any part of the germinal vesicle that contains the chromatin material, such containers being either the nucleolus or nuclear reticulum or both, and that nothing in this research supports the theory of individuality of the chromosomes. The same author ("The Spermatogenesis in *Aplopus mayeri*") finds an accessory chromosome in the phasmid studied and believes the "history of this accessory chromosome gives evidence that it at least possessed a strict morphological and probably also a physiological individuality." One would like to know whether this statement implies that there might be

a morphological without a corresponding physiological individuality. If such an individuality is implied it is a queer kind of individuality, or would be for any other bodies than chromosomes. But chromosomes have had so many queer things attributed to them that queerness with them has almost ceased to be queer.

In a third paper ("The Relation of the Nucleolus to the Chromosomes in the primary Oöcyte of *Asterias forbsii*") the same author expresses the belief that "all the hereditary elements are persistently held by the chromosomes . . . and that these merely receive nutritive material from the nucleolus." It may be granted that the figures and descriptions presented show material to be transferred from the nucleolus to the chromosomes. What the evidence is that this material is all nutritive and none of it hereditary would be extremely important. Certainly no such evidence is presented in this paper.

The fourth paper that contains cytological matter is "The Habits and Early Development of *Linerges mercurius*," by E. G. Conklin. The egg of this medusa presents concentric layers, the outermost of which is protoplasm nearly free from yolk. As in the eggs of various other cœlenterates, cell-division, at least the first division, begins in this peripheral layer; but contrary to what has been held for some other species, the nuclei and chromosomes are here somewhat distant from the point at which the first visible changes toward division occur. There seems, consequently, no observational ground for supposing that this outer layer does not actually start up the division. This would appear to be a very significant point. Conklin gets no evidence that cell-division is ever amitotic in this species as it has been reported to be in one or two other cœlenterates.

Normal Development: W. K. Brooks and B. McGlone show ("The Origin of the Lung of Ampullaria") that in the pulmonate studied, the lung seems to be quite a different structure from that of other pulmonates and hence the conclusion is reached that "there is no reason to think that there is any ancestral connection or relation between the lung of Ampullaria and that of the pulminates." In the paper by Conklin noted under cytology, it is shown that gastrulation in the medusa studied usually takes place by invagination, but sometimes by the immigration of a mass of endoderm cells at the vegetal pole, and the author remarks on the close relationship between the

two processes. The off-hand way in which this fact is now treated, as compared with the almost frantic contention of twenty years ago that the two are fundamentally different, at least in phylogenetic significance, may well be reflected upon when our minds are turned toward theoretical biology.

Regeneration: Under this head there are two valuable papers. One ("An Experimental Study of the Rate of Regeneration in *Cassiopea xamachana*") is by Chas. R. Stockard, and the other ("Some Internal Factors concerned with the Regeneration of the Chelæ of the Gulf-weed Crab") is by Chas. Zeleny. Stockard finds no support in this research for the hypothesis that activity of the regenerating part accelerates or influences in any special way the regeneration. By cutting pieces of various shapes and sizes from the bell of the medusa, he gets the interesting result that on the animal itself the "regeneration rate is fastest from the portion from which most tissue has been removed"; and on the pieces cut away regeneration is "fastest from the part from which the least tissue has been removed." The reviewer would raise the inquiry, Does not this conclusion say essentially that the removed tissues of *Cassiopea* are replaced in the way necessary to effect the quickest and surest restoration of the original form of the animal regardless of the form and place of the cut?

Zeleny's researches were directed at two fundamental points: "The quantitative determination (1) of the effect of successive removal of an organ upon its power to regenerate and (2) of the character of the changes, if any, produced in the uninjured parts of the animal by such removal." The summarized results are: (1) "When the correction for change in power of regeneration with size or age is made it is found that successive removal neither retards nor accelerates the regeneration of the right chela." The criticism may be ventured that this conclusion is too unqualified for the number of tests made, there having been but three removals of the same chela in the same individual. As we commit ourselves more and more to quantitative methods in biology, we shall see more and more clearly, so it appears, the importance of the principle of "large numbers." On the second object of the research the result was: "The removal and regeneration of the right chela produces no change in the growth of the uninjured left chela."

Faunal Zoology: The titles belonging primarily under this head are: "The Pelagic Tunicata of the Gulf Stream," by W.

K. Brooks; "Notes on the Medusæ of the Western Atlantic," by H. F. Perkins; "Helminth Fauna of the Dry Tortugas," by Edward Linton; "A Variety of *Anisonema vitrea*," by C. H. Edmondson.

A sad interest attaches to the paper by Professor Brooks, it having been published after its author's death. Fitting indeed it is that some of the last works from Brooks's masterly hand should be on the group of animals on which his most distinguished observational researches were made. A few unimportant inadvertencies occur in the paper due to the fact that illness prevented the author from putting his manuscript in final form for publication. Some of the figures of Plate I are labeled *S. florida*, and some *S. floridana*. *Floridana* is the right name. The title is somewhat misleading since no general treatment of the tunicate fauna of the Gulf Stream is contained in the paper. As a matter of fact "observations on certain morphological points in the subgenus *Cyclosalpa*" would have been a better title for the first and second parts of the paper. Perhaps the most important general point discussed by Professor Brooks is that of the similarity between the muscle bands of *Salpa* and *Doliolum*. He had previously tried to dispel the erroneous distinction, as he believed, between the two that is suggested by the terms *Hemimyaria* as applied to *Salpa*, and *Cyclomyaria* as applied to *Doliolum*.

In a third section of the paper written in collaboration with Carl Kellner, a new appendicularian, *Oekopleura tortugensis*, is described. Some "Notes on Embryology" add a little to our meager knowledge of the development of this group of animals.

Perkins's paper on Medusæ is by no means a faunistic study in the narrow sense, it containing quite as much that would come under the head of animal behavior as under that of faunal zoology. One of the new species, *Cladonema mayeri*, is treated at length, not only the hydroid, and medusa forms being fully described, but as well various activities and attitudes being dwelt upon in a lively, appreciative manner. There may be a little danger in speaking of a jelly-fish as "evinced the keenest interest in the prospect of a meal," but such expression has at least the merit of recognizing the coordinated though complex and characterizing activities of the organism under a special stimulus; and at the present time the tendency is to allow lower organisms too little rather than too much of psychic life.

Efforts to secure embryos of *Cassiopea xamachana* were unavailing, no males being found even though over one hundred individuals were examined!

Linton's studies on parasitic worms lead him to the conclusion that generally these organisms are not as abundant either in species or individuals, for an equal number of hosts (fishes) in tropical as in northern waters.

An interesting case of identification is mentioned by Linton. The spiral valve of some shark, with its contents, came to him for study. From the entozoa present, taken with the other intestinal contents, he concluded that the organ belonged to *Galeocерdo tigrinus*. It was later found that the jaws of the specimen had been saved. Examination of these proved the original identification to have been right. Question: Where were the "determinants" of the characters by which this identification was made? Were they in the germ-cells of the shark or in those of the entozoa that inhabited the shark's intestine? Of course the case would present no difficulty to a consistent determinatist because the determinant doctrine is founded (unwittingly) exactly on such an *a priori* basis that observed facts can not touch it. As well expect to hurt a ghost with a charge of buck shot, as the determinant theory with objective facts.

Several new species and genera of endoparasitic worms are described in the paper.

Animal Behavior: Under this head come the largest number of titles and, on the whole, probably the most important observations contained in the volumes. This is as it should be, coming, as the studies do, from a laboratory located by design in a peculiarly rich zoological region that is at the same time remote from the great centers of scientific activity. The titles are: "The Annual Breeding-Swarm of the Atlantic Palolo," by A. G. Mayer; "Rhythmical Pulsation in Seyphomedusæ," by A. G. Mayer; "Habits, Reactions and Associations in *Ocypoda arenaria*," by R. P. Cowles; "Habits, Reactions and Mating Instincts of the Walking Stick, *Aplopus mayeri*," by C. R. Stockard; "A Contribution to the Life-History of the Booby and Man-o'-War Bird," by F. M. Chapman; "The Behavior of Noddy and Sooty Terns," by J. B. Watson; and "An Experimental Field-study of Warning Coloration in Coral-reef Fishes," by Jacob Reighard.

As already indicated, some of the papers noticed under other

heads contain interesting matter that belongs here. This is especially true of Conklin's paper on Linerges. It is practically out of the question to give within the limits of a brief review an adequate summary of the contents of this group of papers. The Atlantic Palolo has been under observation by Dr. Mayer for nine years, and various interesting details in the "swarming" of this species have been made out. Besides the principal swarm (near the last quarter of the moon, between June 29 and July 28) there may be a few smaller swarms before and after this. Experiments indicate that moonlight falling on the rocks in which the worms live is indispensable to the swarming.

Mayer's studies on the pulsation of the medusa of *Cassiopea xamachana* is a continuation of work of his previously reported one. The idea that sea water is a balanced solution for this species, and that the animal manufactures its own stimulant to the rhythmical contractions by the "constant formation of sodium oxalate in the terminal entodermal cells of the marginal sense-organs," is still further dwelt upon. Mayer concludes, in agreement with some other observers working on other animals, that in this species, "conductivity of the subumbrella tissue is independent of contractility. Dr. Cowles's article on the "sand-crab" is especially distinguished at first sight by some of the admirable illustrations it contains. It is shown that this crab, in which one chela is decidedly larger than the other, almost always digs its burrow with the side having the smaller chela and enters the burrow after it is dug, with that side in the lead. This, Dr. Cowles remarks, is probably advantageous to the animal in that the larger chela is in the more favorable position for defense. It would seem that this case might have an interesting bearing on the question whether structures take their characteristic forms to meet demands imposed upon them or are used to the best advantage for the organism after they are in existence. In which service are the chela exercised more, in digging or defending and capturing? Possibly the question might be answered observationally. The investigator concludes that the crabs do not distinguish colors visually and do not hear in the ordinary sense; that they see outlines; that the so-called auditory organs are equilibrating organs; that the tactile sense is well developed; and that the animals have memory and profit by experience, and form habits.

The peculiar structure and habits of the walking-stick,

Aplopous, afford the animals protection in a high degree according to Stockard's investigations. Under certain circumstances when the antennæ are removed the "forelegs are readily pressed into service as feelers." Males were found to copulate even in a dark room with the amputated abdomina of the females.

Chapman gives some interesting information on the domestic and civil polity of the booby and the man-o'-war bird. For example, the male and female booby seem to change off in their home duties, the one staying with the nest for a time while the other fishes. Each pair of birds is closely limited to its own small nest area during the breeding time, the rule being enforced by prompt action on the part of members of the colony generally if a particular individual ventures outside his own precincts. The booby seems to have the habit of laying two eggs, only one of which yields a bird. Although not belonging properly under the present heading, mention may be made of Chapman's observation on the order in which the feathers of pteryæ appear and the rate at which they grow. This seems to the reviewer an important subject and one deserving more attention than it has received.

Without minimizing the value of the papers so far noticed, the two still unnoticed, namely those of Watson and Reighard, are probably the most valuable of the whole collection, judged by the number and character of the observations recorded. In his introductory remarks Professor Watson refers to his work as preliminary and speaks with some doubt about the possibility of its being continued. From the beginnings made on several problems of the utmost interest, it is greatly to be hoped that the Tortugas Laboratory will see to it as one of its first concerns that these investigations are kept up. To get some clear light on the one problem of how terns which seemingly have never been over the ground before, can find their way from Cape Hatteras to the Bird Key, something like a thousand miles, would amply justify the expenditure of the institution's entire income for a number of years were so heavy an outlay necessary. Fortunately the work would probably not be greatly expensive. Biologists will do well to recognize that exactly in such phenomena as these occultism and superstition have their strongest roots to-day, and that these roots are by no means frail and sickly. Any victory that science can win in these frontier regions counts for more toward the general enlightenment of

mankind than many won in already well cultivated conquered territory. For the many facts brought out on the relation of the sexes during the breeding period; on the care of progeny; on the instincts and habits of the young; on the intelligence, individual and comparative, of the two species studied; and many other topics, the paper itself must be consulted.

Reighard's investigation being in a field that has long been a storm-center of theoretical biology, will probably attract more readers than any other one in the volumes. In the reviewer's opinion it will, too, exercise a wide and beneficent influence toward rectifying one of the most remarkable aberrations to which scientific speculation has been subject in any domain of science for many a decade. It was the reviewer's privilege a few years ago to spend some time observing the fishes about the coral-reefs of the Hawaiian Islands. From this experience as well as from various others more or less kindred, he is convinced of the essential soundness of Reighard's results. More than that, he is convinced that any naturalist, the windows of whose mind are not darkened with the heaviest screens of adverse dogma, would be likewise convinced were he to examine the evidence for himself.

We must be content with a single quotation from this paper:

Coral-reef fishes are not conspicuous because they are in the reefs; they are in the reefs because they are conspicuous and can not therefore leave the reefs, and because, being in the reefs and taking food as they do, there is no reason for their being inconspicuous. The reefs condition their conspicuousness; they are in no sense its cause.

We should certainly want more light than the author has given on the meaning of the statement that reefs condition but in no sense cause the conspicuousness of the fish. But passing this as of minor importance it is to the first part of the statement that we turn for the real meat of the case. If it be really true, and we recognize the truth, that the color of these fishes has arisen we know not how, but that having become thus elaborate and conspicuous, the fishes find protection as well as food among the corals, then are we at the threshold of the problem of how the color has arisen, with our senses and wits open to receive evidence of whatever sort bearing on the problem. The unfortunate thing about the natural selection theory has been not so much the error it contains as the fact that it has been made an *absolutist theory*; one of the sort, that is, that *shuts the*

door in the face of inductive science. It is time to be undeceived in this matter.

WM. E. RITTER.

LA JOLLA, CALIF.,
Sept. 20, 1909.

EXPERIMENTAL ZOOLOGY

Inheritance of Color in Pigeons.—The breeding experiments of pigeons, described by R. Staples-Brown in the *Proceedings of the Zoological Society of London* for 1908, pages 67–104, are of interest because he repeated certain experiments by which Darwin obtained a reversionary type resembling *Columba livia*. Darwin concluded that domesticated pigeons sprang therefore from the wild rock pigeon. The varieties of pigeons used by Staples-Brown were with one exception identical with those used by Darwin and the experiments were planned on similar lines. Darwin's experiment, it will be recalled, was as follows:¹ A black Barb was crossed with a white Fantail and a black Barb with a red Spot (white bird with tail and tail coverts red having a red spot on the forehead). Upon mating hybrids produced by these two crosses Darwin obtained a pigeon identical with *Columba livia*, excepting that "the head was tinted with a shade of red, evidently derived from the Spot, and was a paler blue than in the rock pigeon." Staples-Brown substituted a black and white Nun for Darwin's red Spot which was not readily obtainable.

A Nun is a white bird with certain well-defined markings of black, blue, red or yellow. The individuals used in Staples-Brown experiments were black and white, the black appearing on the chin and throat, part of the outer flight feathers, a few secondaries and tertiaries, the tail and upper and under tail coverts. The Barb pigeon is self-colored, black, red, yellow, dun or white, with a small beak and the skin around the eyes broad and carunculated. Black Barbs only were used in these experiments. The Fantails used were white. When the Barb \times Fantail hybrids were bred to the Barb \times Nun hybrids no reversionary types were produced. These experiments were discontinued owing to lack of space. However, when Barb \times Fantail hybrids were mated together some of the offspring had blue feathers. The blue color was chiefly on the tail as described by

¹"Animals and Plants under Domestication," 2d edition, Vol. I, p. 209.

Darwin. A black tail bar was present and the wing coverts and back were of a smoky black color, obscuring the wing bars which are common to the rock pigeon. The reversionary type in some cases appeared in the F_1 generation. A similar result was obtained by Darwin in the F_1 generation from a Nun and a red Tumbler.

The author explains the fact of the reversionary blue not appearing until the F_2 generation in the Barb \times Fantail cross, by assuming that the F_1 individuals contain some element which prevents the appearance of blue. This, he holds, is the factor for black self color, which is epistatic to blue. In the recombination of factors in F_2 , those combinations containing blue without the black factor produce individuals with blue feathers, while those with the black factor exhibit black feathers but no blue ones.

Reference is made to other cases of reversion (sweet Peas and Stocks) in which its occurrence is due to the meeting of complementary factors. He adds, "In the case of the Barb \times Fantail cross the evidence is not yet sufficient to show whether the factors needed to produce the atavistic condition are all present in the Barb and their effect merely hidden by the presence of the black factor, or whether a necessary factor is introduced by the fantail; but the fact that no blues came in the F_2 made from an F_1 (Barb \times Fantail) \times F_1 (Barb \times Nun) distinctly suggests that some factor of the blue did come from the Fantail."

The general result of the Barb \times Fantail experiments shows a dominance of black and blue to white. This dominance, however, is imperfect, as the majority show some white feathers. In the F_2 generation from such crosses, the following types were obtained:

- Black.
- Black with some white feathers.
- Blue.
- Blue with some white feathers.
- Red.
- White with some colored feathers.
- White.

The significance of the presence or absence of the white feathers is obscure. It was first thought that the white feathers indicated that the bird was producing white-bearing gametes. This is not true in all cases.

In matings of blues with white feathers, whites were produced, and a definite proportion of homozygous blues was to be expected. With one exception, however, all the blues produced from these matings showed some white feathers.

Some of these were probably homozygous, but it could not be tested without making use of a large number of such birds. The order of dominance was found to be black, blue, red and white. In minor characters, such as irides, color of beak and claws, the author obtained the following results; white irides dominate black, and there is some evidence of correlation between white iris and black plumage. There was also marked correlation observed between the pigment in the beak and to some extent in the claws, and the plumage. In general, white-plumage birds have white beaks and claws. In the case of eye-wattles, red is dominant over flesh color.

B. B. HORTON.

